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CHRISTOPHER COLUMBUS.

No greater man has received the honor of centenary commemoration than the illustrious discoverer of the new world—Christopher Columbus. In every way he is worthy of grateful remembrance. He was not only a courageous pioneer of discovery, but a man of the noblest character. Not faultless, indeed, but of such a generous nature that his errors are speedily forgiven and forgotten on account of the transcendent luster of his virtues and the incalculable importance of his work. The fetes at Genoa and at Huelva, which are taking place this month, are but the beginning of still grander international festivities in his honor, in which it is regrettable that England, which perhaps more than any other country has benefited by the enterprise of this amazing man, does not participate more prominently. To him we owe our West Indian possessions and those vast colonies, the majority of which have passed from our dominion, but whose inhabitants are destined to continue the traditions and glories of our race under exceptional circumstances of combined freedom and boundless prosperity. Surely, then, should we share in honoring the memory of the man who, like Shakespeare, by reason of the splendor of his services to humanity, is entitled to be considered as among those who are claimed by the entire human family without distinction of race or country.

ALLEGED PORTRAIT OF CHRISTOPHER COLUMBUS.

(From a Contemporary Painting now in the Town Hall, Genoa.)

Many errors concerning Columbus have been rectified within the past few years. It is no longer reasonable to disguise the place of his birth, or the precise position of his family in the social scale. He was born about the year 1438, not in Genoa, but in the neighboring town of Savona, where, according to documents recently discovered by that indefatigable historian, the Marquis Staglieno, of Genoa—documents verified in every particular by the writer of this article—his family, for reasons of economy, had taken a house, while their own in Genoa itself was let to friends possibly in the same line of business as themselves. The family of Columbus, who were woolen manufacturers and dyers, and not wool carders, as has been hitherto stated, did not remain long in Savona, and their illustrious child, who was the eldest of four children, was brought to Genoa, presumably when he had attained his fourth year. The family lived in an ancient house, still standing in the Portorio—or Porta Aurea—a long, steep street leading from the gate of St. Andrea to the church of St. Stefano, and mostly inhabited by merchants engaged in the sale of wool and the manufacture of woolen materials. Not far from them lived in the same thoroughfare, for instance, the dyer Paul of Novi, who subsequently became Doge of Genoa, and the documents recently discovered in Genoa, which threw much light on the early years of Columbus, prove satisfactorily that whatever property the family possessed belonged to the mother, a Fontanarossa,

and the only daughter it would appear of a fairly well-to-do dyer.

Playing about the Port and along that favorite resort of Genoese urchins the Mure della Streghe, or the Witches' Wall, the boy Columbus must soon have become imbued with a love of adventure, and of that sea which stretched before his delighted vision, the deepest and the bluest of all seas, the Mediterranean, across the horizon of which the galleys of the rich republic came and went in mysterious succession, objects of singular attraction and speculation to so imaginative a child. It was an age of mystery and adventure. People were beginning to read more generally than heretofore—and everybody was full of theories concerning the shape of the earth and of the existence of marvelous lands beyond the Pillars of Hercules. We can imagine the child, Christopher, listening with eager face to the tales of the sailors who flocked, then as now, under the dark arches of the Ripa, where in the little cavern-like ships, lighted by the flicker of the lamps burning before the Madonna, they chattered over their wine and business and filled the mind of the impressionable youth with visions of lands, the mountains of which were of gold, and the rivers, literally flowing with milk and honey, flowed over beds paved with silver and pearl. Gradually the determination to discover these happy lands developed in the mind of the boy, and he applied himself to study with rare zeal. We will pass over the well-known story of his early career, pass over the oft-told tale of his vain endeavors to persuade his jealous contemporaries that there was something "beyond human knowledge" about him, and pause for a moment with him at the gate of the little convent of La Rabida. Surely never was there a more pathetic scene than this of the worn-out traveler with the little child sleeping in his arms, pleading for a night's rest in one of the cells of the humble sons of St. Francis. On his reception by the friar hung not only his own fate, but the fate of millions as yet unborn—the discovery of the new world. The traveler is received with open arms, the tired child is laid tenderly to rest, and the cheery voice of the worthy Prior Juan Perez bids Columbus a cordial and a practical welcome. He introduces him to the enterprising brothers Pinzons, who aided him so considerably with their wealth in his gigantic enterprise. The convent of La Rabida is one of the most important places mentioned in the world's history, and as we write there is an idea of erecting near the old shrine a splendid temple dedicated to God, to the patron saint of Columbus, St. Christopher, in the expenses of which the peoples of all nations and creeds are to be invited to share.

In the beginning of August, 1492, four hundred years ago, the little fleet of three vessels—the Santa Maria, the Pinta, and the Nina—sailed out of the placid harbor of Palos. Fortunately for posterity, Columbus kept a diary, recording every event which took place on his memorable journey. Needless, therefore, to enter into particulars of a voyage which should be familiar to all.

It is almost impossible, however, for us to conceive the intensity of the mental strain endured by Columbus from the hour he left Palos until the memorable morning of October 12, 1492, when at sunrise the beautiful island of San Salvador rose to the amazed vision of the discoverer and his companions, like a glittering emerald out of the quiet waters of the West Indian Archipelago. The feelings of the crew now gave vent to most extravagant transports of joy. They who had hitherto considered themselves the victims of an ambitious visionary, who was hurrying them to inevitable destruction, now looked upon themselves as the favorites of fortune, and their captain as a sort of demi-god. With that profound feeling which had always distinguished him, Columbus landed and took possession of the lovely island in the name of his Saviour, and called the place San Salvador. Alas! for the gentle inhabitants of these fair islands, they had not the amiable Columbus alone to deal with, and the subsequent story of their long martyrdom and extinction throws a gloom over the brilliance of the adventures which led to their introduction to the old world. It is certain that Columbus, if he landed at all during his voyages on the coast of the continent of America, did so without knowledge of its importance, and at a place on the coast of British Honduras which has been for centuries associated, by local tradition, with his visit. He, however, to his last day, was under the impression that he had only touched upon the outlying islands which he supposed skirted the East Indies.

The triumphant return to Spain and the splendid reception accorded the illustrious man and his followers by Ferdinand and Isabella, at Granada, and in the fairy-like palace of the recently conquered Moorish kings, is an event too well known to need recapitulation. It is enough to say that, whereas the crafty father of our Katherine of Aragon figures most unenviably in the story of Columbus, the generosity of his consort, Queen Isabella, has immortalized her name and associated it forever with that of the great Genoese, who, kneeling at her feet, besought her to accept the newly-found "Indies" as a gift worthy of her magnanimity.

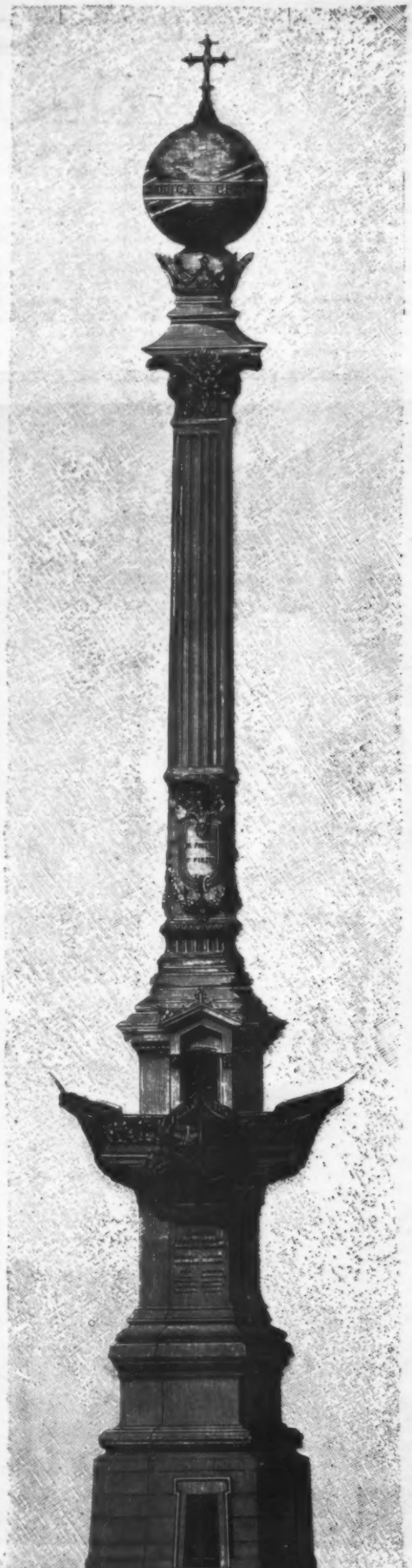
The end of Columbus' life was inexpressibly sad. He was still in the vigor of his life when, worn out by fatigue, weighed down with sorrow, abandoned by friends and even by foes alike, he fell into a languishing sickness at Valladolid in a house still pointed out as the scene of his death. The crafty Ferdinand beheld with calculating coldness this generous man sinking into a premature grave, the victim of that deferred hope which "maketh the heart sick." A little more disappointment, a little more neglect, a little more insolence, the evil king seems to have thought, and this strong man will be broken like a poor reed.

Yet even on his death pallet Columbus was worthy of himself. He received with angelic meekness the lesson of his life. He turned with sublime confidence from men who treated him with contumely to God, to Christ, and passed away murmuring the very words, "In manus tuas, commendo spiritum meum," which Christ spoke in agony on his cross. The only bitter feeling which he permitted to shadow his last hours was not one of anger and contempt for his foolish enemies, but of remorse for his thoughtless conduct toward a too confiding woman. He had never married Dona Beatrix Henriquez, the mother of his beloved son Ferdinand, and the recollection of this unlawful attachment filled his dying hours with, to use his own expression, weighty remorse and "heaviness of soul."

His will is a monument to his character. Every item speaks for his disinterestedness, humility, sweetness of nature and charity. Each line is pregnant with the spirit of resignation and justice. No one is forgotten—even the poor Jew of Lisbon, from whom years before he had borrowed a silver mark.

After a very minute study of the existing portraits of Columbus, I am of opinion that no likeness of him, painted from life, exists; unless, indeed, it be the faded little picture in the Cluny Museum, which was painted, it is said, for Francis I. The reason for my making this perhaps startling statement is that not one of these portraits, and I have seen the greater number of them is in the style of contemporary art. Moreover, the early years of Columbus were passed in obscurity, and he was only for a few months in a sufficiently prominent position to warrant his portrait being taken by any artist of distinction. There is no mention in the contemporary documents of any portrait ever having been painted of him. Still a number of pictures exist of him, which were possibly painted soon after his death, and these are distinguished by certain features which are unmistakable. We are assured by his contemporaries that he was a tall, well-shaped man, with good features, red hair, fair complexion, and blue eyes.

May I in conclusion add a few words in favor of the proposal to erect in the island of Jamaica, as a memorial in honor of Columbus, an observatory of marine biology, at a point which is of the greatest interest to naturalists? The waters of the West Indian seas teem with animal life, little known even to the most advanced naturalists. Sir Henry and Lady Blake have taken a practical interest in this matter, and no more fitting monument could possibly be erected in the West Indies to the lasting memory of one to whom science owes so much. Particulars of this project can be obtained from her grace the Duchess of St. Albans, who is actively engaged in the promotion of the scheme, and also from Mr. H. Villiers Stuart.—*Richard Davey, in Black and White.*



COMMEMORATIVE COLUMBIAN MONUMENT ERECTED IN FRONT OF THE CONVENT OF LA RABIDA.

HUBERT HOWE BANCROFT.*

THE ethnological and historical work performed by Mr. Bancroft is remarkable, primarily, on account of the freshness of the field and the application of practical and scientific methods to the literary reduction of an otherwise refractory mass. In every great achievement may be found two principal motive powers—individuality or unity in the planning and directing, and the utilization of the labor of others. What city could be built, what government or business of importance could be carried on, by one man alone? Hitherto it has been deemed well nigh impossible to unite in extensive literary work these two essentials. "I must know every point, I must verify every statement," says the old-time writer. As well might the railway builder say, "I must lay every tie, I must drive every spike myself." Mr. Bancroft was the first to apply to historical work, to any considerable extent, the talents of others; since which time, however, his method has been adopted by the very men who first ridiculed it; and it is safe to say that no great work of this kind will ever hereafter be performed by a single, unaided individual writer.

True, it is not to be expected that operations of the kind will often be undertaken or executed on such a grand scale as that achieved by Mr. Bancroft. It is doubtful if such conditions will ever again unite in one man—the desire, the ability, and the means. No government or society even will do such work, lacking, as such associations do, unity of purpose over a long period of time, and the apparently extravagant outlay of means.

Mr. Bancroft took for his field an area equivalent to one-twelfth of the earth's surface, and one of the fairest portions of the planet, on which is destined to develop the highest culture. Time enough had elapsed for civilization to have had here a beginning, but not so much that all historical events could be thoroughly ascertained and traced. There were gaps, it is true, in the written and printed records, but these the author and artificer filled in from the mouth of living witnesses, dictations from thousands of whom he took, personally and by the hands of assistants and secretaries. All together the material he gathered, printed and in manuscript, numbering between fifty and sixty thousand volumes, is the largest collection of American history extant, and the largest collection on any one subject or area in the world. To reduce this mass—

* With a portrait, from the *American Antiquarian.*

the work of a dozen lifetimes—and bring it within control, within the period of one lifetime, took twenty men working continuously for twenty years. For twenty years Mr. Bancroft's library or literary workshop was not closed for a single day, except holidays and Sundays, being regularly opened each day at eight o'clock and closed at six. Elaborate catalogues and indexes were made, involving the labor of years, to direct the writer to the required information. The grand result of library and literary efforts of author and assistants is a matchless work, or series of works, in 39 volumes.

The ethnological part, to which the first five volumes are devoted, embodies all that was known or could be ascertained of the many aboriginal people inhabiting the vast territory covered by the work at the time they were first encountered by Europeans. The work is part of and preliminary to the history. Original investigation was not here attempted, as in the making of the history proper, but the results of the labors of thousands of competent scholars and observers are here brought together in a manner never before accomplished.

Mr. Bancroft's work is done. All that he undertook he accomplished. What he did was of the greatest magnitude, and is, and always will be, of the highest importance to mankind, constituting as it ever will throughout all time the corner stone of the history of one of the most important sections of the globe.

Judge Albion W. Tourgee says: "Mr. Bancroft's story of the Mexican revolution is the finest picture of an epoch that exists among English historians. I might have excepted Macaulay's, but that I know his to be a lie; for he puts in anything to produce lights and shadows. The revolution in Mexico was considered nothing but internal broils. Mr. Bancroft exalts it into the grandest and most dramatic struggle that ever existed. No man outside any other nation ever did so much for any people as H. H. Bancroft has done for Mexico, and the people should appreciate it and erect a statue to him. No other historian ever did so much for any people. Grote and Gibbon wrote the histories of Greece and Rome, but then histories had already been written and these men could not make those countries any greater. With Bancroft it was different. He took Mexico out of oblivion and exalted it into a great nation. I rarely read a book more than once, but this study of the Mexican revolution I have read two or three times, and not for any special purpose, but merely for the pleasure its reading gives me. I reiterate, Hubert H. Bancroft has made Mexico heroic."

The sketch of the life of Mr. H. H. Bancroft, of San Francisco, has been given to the public in a very readable little volume, called "Literary Industries." The book reads like a romance, and is especially interesting to literary men, for it shows what possibilities are hidden away in the human mind, needing only the opportunity and encouragement to develop great success. We recognize in this case the influence of early literary surroundings, which continue even while great business responsibilities were uppermost, but finally culminated in the choice of a special work, to which this author gave twenty of the best years of his life.

Mr. Bancroft was born in the little village of Granville, Ohio, on the 5th of May, 1832. It was a staid village, with Puritan ideas, but the pranks of some of the young fellows were in strong contrast with the teachings of their homes. The book business was the chosen occupation of Mr. Bancroft. The firm of Derby & Co., of Buffalo, were his first employers. His apprenticeship was not altogether agreeable, but resulted in a discipline which was valuable to him afterward. While still a young man he made a venture in selling books for himself which proved successful. This encouraged him to start for California in 1852, and to embark in the book business in the new and growing city of San Francisco. In this business he was assisted by his brother-in-law, Mr. Derby, of Buffalo, and afterward by his brother, Mr. A. L. Bancroft, who gave his name to the house, the firm being known as A. L. Bancroft & Co. The business extended from British Columbia to Mexico, and to the Hawaiian Islands, to Japan and China. In the year 1859 the firm was about to publish a hand-book almanac. It occurred to him that it would be convenient to have the books on California, Washington, Oregon, and Utah together. He accordingly selected them from the stock, and found that they formed a little library of from fifty to seventy-five volumes. The beginning of the Bancroft library was as follows: Mr. Bancroft continued to collect, but in 1862 had only a thousand volumes on hand. A visit to London and Paris opened his eyes to the immense number of books on the subject and to the value of such a collection. His first large purchase was that of Maximilian's library, which was done by telegraph, on January 16, 1869. In 1876 he purchased the collection of E. G. Squier, in 1879 the library of Cushing, of Boston, in 1880 the library of Ramirez. The books ordered cost several hundreds of dollars. The library was first placed in the second story of the book store. It was afterward moved to a building on Valencia Street, and this became his workshop.

HUBERT HOWE BANCROFT.

The idea of writing had not yet dawned upon his mind. The work of collecting was conducted as a business transaction, without a thought as to how the books might be utilized. The difference between Mr. Bancroft and the majority of book collectors was this: he was not exclusive in his tastes, but, on the other hand, took the public into his confidence. It was in this way that the thought came to him that he might make the library the basis for a work of some kind—either a cyclopedia or a history—which should embrace the various provinces and States situated on the Pacific. A literary recluse would never have conceived the idea of gathering such a library; a selfish business man would never have adopted the plan of doing literary work, but the ambition of Mr. Bancroft which led to the establishment of the library afterward led him to write the history.

It would have been a great work, and a timely one, if he had merely gathered the manuscripts which had been written, and which were likely to be destroyed, and put them on file for future use. It was a still greater work to buy all of the books which had been published in different languages relating to the different provinces on the Pacific Coast. But the greatest of all was the work which he soon began—that of writing a series of books which should be both cyclopedia and history, and which should place before the public the rich treasures of literature contained on the shelves. It was really embodying the library into a series of bound books and distributing it over the two continents.

The plan was a large one, but was well conceived. Other persons may have carried out the details of the work, but the selecting of those persons and the organizing into an efficient and well trained band, and furnishing them with the means to carry out the project, was Mr. Bancroft's personal mission. He entered into it with great enthusiasm and cheerfulness. This idea grew upon him until he came to feel that his life was sacred and must be preserved until his mission was carried out.

THE CONVENT OF LA RABIDA.

It was a prodigious undertaking, and at times became even oppressive from its very magnitude. It seemed at first as if he would be swamped by the very amount of the material he had gathered. Chaos and confusion would have come to his mind if he had undertaken to carry out the details; but he was, with his money, able to employ capable men to assist him. These men went through the library two or three times, indexing and classifying the books and afterward made copious notes and quotations. For his own improvement and for the cultivation of style, Mr. Bancroft went outside of the library proper and took such miscellany as Buckle, Spencer and other writers upon civilization and the growth of society. This course was a wise one. It gave vivacity, comprehensiveness and interest to his books, and brought the old into contact with the new. There was no such thing as burying this man of affairs under the heaps of musty books and old manuscripts, for he was obliged to leave his seat in the library and go to his book store and superintend the business of the firm. His literary industry was great. As Charles Sumner said of himself, he often came to his "second breath" and kept up the pace of a rapid and diligent writer for weeks and months, continuing through the twenty years in the exercise of his faculties and enjoying to the utmost the very activity of a literary man; but finding the rest and variety which business and travel brought to his mind.

Mr. Bancroft was very happy in his companionships. His wife was a companion to him in his studies and frequently attended him in his journeys, and assisted him in gathering and securing his manuscripts and books, which were scattered about in different places throughout the Pacific Coast. An Italian named Enrique Cerruti, who could speak several languages fluently, was very devoted to his service, and accomplished much in the way of collecting rare manuscripts. It is greatly to the credit of the gentlemen who had written local histories that they should give the results of their labor to Mr. Bancroft so generously and do all they could to further his enterprise. Among these may be mentioned Gen. Vallejo, who had at his residence priceless documents relating to the history of California. J. A. Sutter also furnished some valuable facts and spent much time in writing the history of the State. Mr. James G. Swan, Mr. Stephen Powers, and Mr. Pinart put into his hands valuable documents about Alaska. The assistants of Mr. Bancroft were men of great ability and industry. Among these may be mentioned Mr. Henry L. Oak, a native of New England, who did a large part of the indexing; Mr. Savage, who gathered much material about California; Judge Hayes and Edward F. Murray, of Los Angeles; Mr. Thomas F. Long, of Vancouver Island; Mr. A. C. Anderson, of Victoria; Mrs. F. F. Victor, of Oregon, who was engaged in the library for many years. Mr. Pettroff translated much of the Russian literature, and labored in the library on the history of Alaska. The system of note taking was supervised by Mr. Nemos.

Mr. Bancroft experienced some reverses. He was just in the midst of the publishing of his books when a fire broke out in his store. All of his very heavy

stock of books was consumed, many of the stereotype plates of his histories were destroyed, with a large quantity of paper. For a time it was a question whether the firm could survive such a calamity. Fortunately the library had been placed in a separate building and was safe. This library is still in California and should be kept there, for it really belongs to the State.—*The American Antiquarian.*

BURNS MEMORIAL STATUE, AYR.

WE give an illustration this week of the statue which has been erected to the memory of Robert Burns, in the town of Ayr, with which he was so closely connected, and which he has commemorated in one of his most celebrated poems by the flattering couplet:

"Auld Ayr, whom ne'er a town surpasses
For honest men and bonnie lasses."

The statue is by an eminent Scottish sculptor—Scottish by birth though now English by residence and adoption—Mr. G. A. Lawson, who has studied various portraits for the likeness, and has adopted a costume such as was probably worn by Scotchmen of Burns' rank of life in his day. Another lithograph gives the architect's detail drawing of the pedestal, which was exhibited at the architectural room of the Royal Academy this year, and is an instance, of which we hope we shall see more in this country, of proper thought and design being bestowed on the treatment of a pedestal for sculpture. It was designed by Messrs. Morris & Hunter, architects, and executed in granite from the Kemnay Quarries by Mr. Taylor, Aberdeen. In height it is 12 ft. 3 in., the size of the base being 12 ft. The lower part of the die has a space on each side for a bronze bass-relief, to consist of subjects from the poet's works. Between the die and the main cornice there is a sculptured frieze, worked from models prepared by Mr. David M'Gill, gold medalist, South Kensington, who is a native of Kilmarnock. The frieze is in the form of a ribbon springing from a serpent, the symbol of eternity. This ribbon, which is carried uninterruptedly round the pedestal, has cut upon it the names of all the places at which Burns at various times found a home, beginning with Alloway in 1759, and going on to Mount Oliphant, Lochlee, Kirkoswald, Mossgiel, Edinburgh, Ellisland, and finally Dumfries, the period spent in each being indicated by the dates. The decorative background of the frieze is intended to symbolize Burns' power over the English-speaking race. In the front frieze the background represents the thistle of Scotland, in the next the rose and shamrock are combined as representative of England and Ireland, and in the third the palm leaf indicates India and the colonies, while in the fourth the hawthorn or mayflower is utilized to represent the North American continent, which, it seems, has no recognized national floral emblem. In the absence of this desideratum the mayflower has been selected as recalling the close connection of America with this country, which had its beginning with the landing of the Pilgrim Fathers. The statue is 9 ft. 1¼ in. high, the total height of the monument thus being nearly 21½ ft.—*The Builder.*

THE BURNS STATUE, AYR.—MR. G. A. LAWSON, SCULPTOR.

THE PROGRESS MADE IN TEACHING DEAF CHILDREN TO READ LIPS AND TALK, IN THE UNITED STATES AND CANADA.*

By ALEXANDER GRAHAM BELL.

THE total number of teachers of the deaf employed in the United States in 1890 was 641, and in 1891, 686. This is an increase of 45. When we come to analyze the details we find that this is an increase exclusively of articulation teachers. This is shown by the following facts. In 1890 there were 213 articulation teachers employed, whereas, in 1891, there were 260, an increase of 47 articulation teachers. The first statistics upon this subject were collected by the Annals in 1886. In that year we find articulation teachers constituted 32·8 per cent. of the hearing teachers in our schools for the deaf. In 1887 they constituted 40·5 per cent.; in 1888, 44 per cent.; in 1889, 45·7 per cent.; in 1890, 45·2 per cent.; in 1891, the latest returns, 50 per cent. Indeed, they constituted one more than 50 per cent. There were 260 articulation teachers to 259 hearing teachers who were not engaged in articulation work.

In regard to the proportion of deaf pupils taught speech, the increase during the past year has been very marked. In 1890, there were 3,682 deaf children in the United States taught speech; in 1891, 4,245, an increase of 563. In 1890, 41·3 per cent. of our pupils were taught speech; in 1891, 46 per cent. I am sure that this increase is due very greatly to the stimulus of the first summer meeting of the American Association to Promote the Teaching of Speech to the Deaf.

Of course, the statistics in the Annals include the whole of our pupils, old as well as young, and it has occurred to me, therefore, that they may not give us a true indication of the extent to which the California resolution is being carried out in the country at large; and that a better indication would be obtained by statistics concerning younger pupils alone. I therefore sent out a circular letter of inquiry to the superintendents and principals of American schools for the deaf, requesting:

1. The total number of new pupils admitted during the school year just closed.
2. The number of new pupils taught speech; and
3. The number of these taught by speech.

Replies have been received from schools containing 7,987 pupils, or 80 per cent. of the whole number under instruction in the United States and Canada.

The following table shows the results of the inquiry:

SPEECH TEACHING IN AMERICAN SCHOOLS FOR THE DEAF, 1891.

Schools for the Deaf.		Number of Pupils in Year 1891 [Annals of January, 1891].			New Pupils Admitted in School Year Ending June, 1892 [Replies to Circular of A. G. Bell].			Percentage.	
		Total pupils.	Total taught speech.	Percentage taught speech.	Total new pupils.	Total taught speech.	Total taught by speech.	Taught speech.	Taught by speech.
United States.	A	5,614	2,900	53	836	580	363	69	43
	B	1,619	759	47	260	180		71	
	C	1,999	596	27					
Total		9,232	4,255	46	1,102	769		70	
Canada.	A	445	92	21	89	20	5	35	8
	B	309	132	43	63	41		65	
	C	39	5	13					
Total		793	229	29	122	61		50	

A. Complete returns were received from these schools.

B. The returns received from these schools did not state definitely the number of new pupils taught by speech. The Canadian schools marked B refer to the two Roman Catholic schools in Montreal. They return 136 pupils, or 41 per cent. of the whole, as taught by speech; but do not state how many of the new pupils were so taught.

C. These schools did not reply to the circular letter of inquiry.

It is encouraging to note that while 46 per cent. of the whole number in our schools last year were taught articulation, 70 per cent. of the younger pupils were afforded an opportunity of learning to speak. The statistics published in the annals are somewhat defective because, while they give us the total number of pupils taught speech, they do not give us the number taught by speech; so that we have no statistics by which we can measure the progress of the oral method of teaching in America.

Professor Joseph C. Gordon, of the National Deaf Mute College, in some editorial remarks prefacing a volume entitled "The Education of the Deaf," about to be issued by the Volta Bureau, says: "The returns of pupils taught by speech are incomplete. The number reported for 1891 is 963, or 10·4 per cent. of the school population." The above table indicates that the percentage, in the case of the younger pupils, must be very much larger. Out of 836 new pupils admitted during the past school year, 363, or 43 per cent., were taught by speech.

This percentage, however, is probably excessive, because the table shows that those schools which have done the most work in articulation teaching have been the most ready to respond to inquiries relating to the subject. We cannot, therefore, assume that the percentage holds for the schools that have not replied to my circular letter.

Still, even if we assume that these 363 deaf children were all who were taught by the oral method, the percentage must be very much higher than that given by Professor Gordon. This will be obvious from the following considerations:

The schools containing these cases had a total attendance of 5,614 pupils, of whom 836, or 15 per cent., were new pupils—admitted during the past school year.

If this proportion held good for the whole country, then there must have been a total of 1,385 new pupils—or 15 per cent. of 9,232—admitted during the year just closed.

Now 363 of these, at least—or 26 per cent.—we know were taught by speech. Hence, for the younger pupils, the true proportion taught by speech lies somewhere between 26 and 43 per cent. of the whole. The lowest estimate very much exceeds the figures of Professor Gordon.

* Address delivered at the conference of the superintendents and principals of the schools for the deaf of North America, held at Colorado Springs, Aug. 8-11.—*Science.*

We have no means of ascertaining whether the proportion of our pupils taught by speech is increasing or diminishing; and I think it would be well to direct the attention of the editor of the Annals to the importance of collecting and publishing statistics upon this subject.

I have glanced over the most recent reports of American schools for the deaf, and there are a few points contained in them to which I shall direct your attention.

In the twelfth biennial report of the American Asylum at Hartford, the principal, Dr. Job Williams, gives his views upon what constitutes success in articulation work. He says:

"We hold that direct and earnest effort should be made, by expert teachers of those branches, to teach speech and speech reading to every pupil, and in no case should that effort be abandoned, until those teachers are convinced that the pupil will never acquire enough of speech to be of any practical use. In some very unpromising cases the possibility of acquiring speech is not given up for two or three years. Here let me say that the criterion of success in speech should not be perfect naturalness of tone and inflection. It would be unreasonable to expect that, where the sense of hearing is wanting. Intelligibility is the prime requisite of good speech. Tone and inflection are secondary considerations. Any pupil who has mastered speech and lip reading so far as to be able to carry on conversation in regard to the ordinary affairs of life in speech so plain as to be readily understood by the members of his own family, even though others fail to understand him, should be counted as a successful articulator and lip reader. It is worth while to continue the instruction in these branches in many cases where the degree of success falls considerably short of the ability to carry on an extended conversation, provided that what of speech is acquired is easily understood. We must recognize the fact that intelligible speech is the readiest and most acceptable means of communication with people in general, but it must be intelligible. It is worth while for a child to gain even a limited amount of speech and lip reading (the latter is as important as the former) in all cases where it can be done without serious sacrifice in mental development and acquisition of language."

We all must agree with Mr. Williams in these remarks. We should, of course, aim to have our pupils speak so clearly and distinctly that any one can understand them; but I am sure Mr. Williams is right in saying that a much lower degree of proficiency might constitute a pupil a successful articulator and speech reader. Mr. Williams rightly claims that oral instruction is successful if the speech of pupils is intelligible to their friends in their own homes, and among their own people, even though others have difficulty in understanding what they say. It is a very difficult thing for a teacher, and especially for an articulation teacher, to realize this. I have been myself a teacher of articulation, and I know how they feel. Their ears are sensitive to mispronunciations, as mine were. It is difficult for them to realize that voices, which to them may be disagreeable in tone, may be very sweet and pleasant to those at home. It is difficult for them to realize that imperfect speech may be better than none at all, and that speech so defective as to be unintelligible to strangers may be of the greatest value to the pupils in their own homes, and among their own people, as a means of communication. This fact has been specially impressed upon my attention by the report of the Mississippi Institution, which, in many respects, is a very remarkable document. That institution has had a class of twelve pupils taught altogether by speech and speech reading. While all of them have made great progress in speech reading, some have gained but little power of speech.

Mr. Dobyns, the principal, says:

"While I have been more than satisfied that the institution was justifiable in the small outlay in this department of instruction, yet, for fear my zeal to keep pace with the times may have gotten the better of my judgment, I submitted the following questions to the parents of the pupils in this class, knowing that they desired the very best thing for their children."

I will not take up your time by reading the questions and replies, but will merely say that the answers demonstrate that speech, which may be thought very little of by the sensitive ear of the teacher, is considered a blessing at home. None of these parents desire their children to be removed from the oral department of the school; but, on the contrary, they all earnestly request that their children be continued in this department. Where there is any difficulty in deciding upon the value and success of the articulation taught to our pupils, with whom should the decision rest? Surely with those who are nearest and dearest to our pupils—with those who have their interest most at heart. Mr. Dobyns, I am sure, is right in referring the question to the parents and friends at home.

In this report, Mr. Dobyns incidentally remarks that now, whenever a new pupil enters the institution, the request comes from the parents: "Please see if you can't teach my child to speak." He has, therefore, asked from the Mississippi Legislature an increase of appropriation to enable him to employ another articulation teacher; and I am sure we all hope he may get it.

There is another point in the report of the Mississippi Institution to which I would direct your attention. Mr. Dobyns has collected and published statistics concerning the earnings of former pupils, and he goes to his State Legislature with the proof that the graduates of his school, so far from being dependent upon the public for support, are actually wealth producers, earning annually a larger amount than the State appropriates for the support of the school. He proves that it is not a matter of charity to educate the deaf; and demonstrates that the money appropriated for this purpose is in the nature of an investment, yielding profitable returns to the State.

I would urge all schools for the deaf to carry out this plan of Mr. Dobyns, and collect statistics concerning the earnings of former pupils. I would suggest that these statistics should be so tabulated as to distinguish the earnings of the pupils who could articulate and read speech from the mouth from those who could not. I have no doubt that pupils who speak have an advantage in life over those who do not; and that statistics will demonstrate that their average earnings exceed the average earnings of those who are unable to articulate. If this should turn out to be the case, what an argument it would be to present to legislatures in favor of appropriations for articulation teaching!

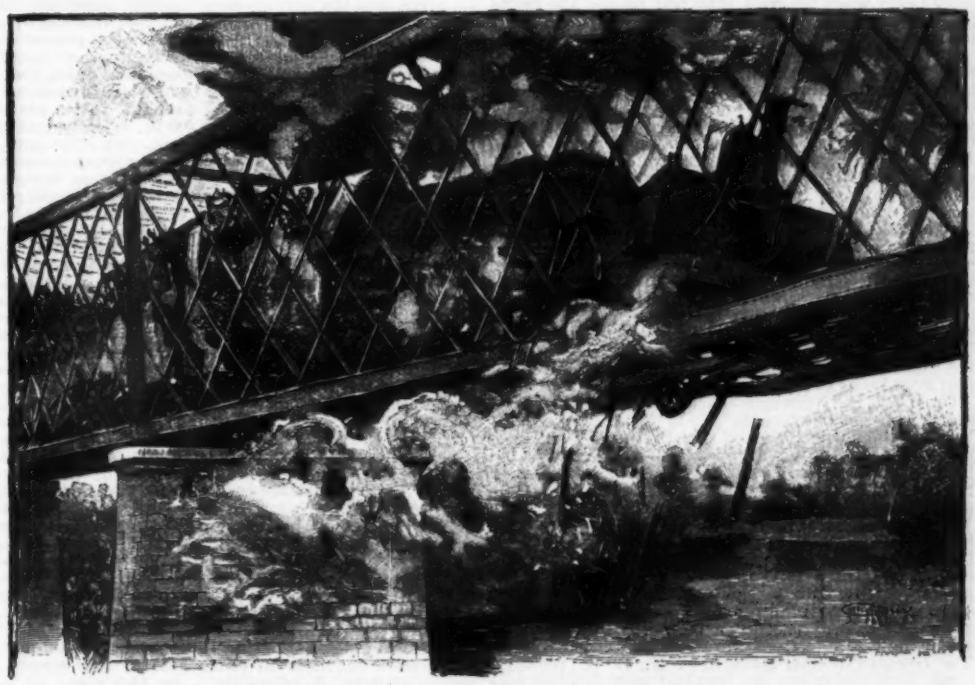
I venture to predict we shall find that our former pupils who speak, even though they may be unable to read speech, earn more per annum than those who are forced to resort exclusively to manual means of communication, and those of them who can read speech, as well as speak, are still better off in life.

Mr. Davidson, of the Pennsylvania Institution, has suggested another valuable line of inquiry. From a comparison of numerous letters in his possession, he makes the assertion that orally-taught pupils improve in their knowledge and use of language after leaving school. I would suggest the importance of preserving uncorrected letters of your pupils during the whole period of their school life, and of keeping up correspondence with them after they leave school. A comparison of letters written by the same pupil at different periods of time would be invaluable as a means of determining his progress, and the correspondence in adult life might be utilized, for the purpose of collecting statistics concerning the earnings and general success in life of our pupils.

THE ACCIDENT AT BEZIERS.

THE accident at Beziers occurred under quite peculiar circumstances. It was, in fact, upon a bridge that crosses the Orb that the locomotive of a train running from Beziers to Cessenan was derailed and thrown across the track. The cars behind it crashed into each other, and several of them were reduced to fragments.

The metallic superstructure of the bridge, fortunately, resisted, otherwise the entire train would have been precipitated into the abyss and the number of victims would have been much larger. As it was, some forty passengers were injured, and seven or eight of them seriously. Four persons were killed outright.



A RAILWAY ACCIDENT IN BELGIUM.

The cause of the catastrophe is attributed to the bad state of the wooden ties upon which the rails are placed. These ties, which were rotten, having given way under the weight of the locomotive, the latter ran off the track, stopped short, and formed an insurmountable obstacle to the cars impelled by the speed acquired.—*L'Illustration.*

ICEBERGS.

ICEBERGS, abandoned ships, and fog are three of the most dreaded hindrances to safe navigation that cumber the restless surface of old ocean. All that can be done to help the toilers of the deep sea is to put in position on comprehensive charts the most reliable limits within which ice is likely to be met with by passing ships; or, better still, to distribute as widely as possible the latest information with respect to icebergs and other dangers. Our hydrographic department issues general charts setting forth the equatorial limits of icebergs, but the invaluable pilot chart of the North Atlantic, issued every month by the United States hydrographic office, has greater exactness to recommend it to navigators crossing that stormy sea, who are supplied with copies thereof without charge. Shipmasters and officers of merchant ships under every flag make careful reports to Washington of the general atmospheric and other phenomena encountered at sea during their passages over various tracks; and the pilot chart, embodying the recorded experiences of many, is published at the beginning of each month to aid individual navigators in determining upon a correct course of action in the trying circumstances in which they are not infrequently placed. No fewer than twenty-five thousand copies of these pilot charts were distributed gratuitously by the Washington authorities by means of branch offices at the large United States ports during the twelve months ended June 30, 1891. Commander Richardson Clover, United States navy, the American hydrographer, is to be congratulated on the international utility of this accurate and well arranged guide for North Atlantic navigators. We are indebted to the supplement of the July chart for some of our information with respect to icebergs drifting about off the coast of Newfoundland and to the eastward thereof this season, endangering the safety of some of our noble transatlantic liners.

The extremities of glaciers entering the sea in high northern latitudes are wrenched off by the upward pressure of the water into which they intrude; and the resulting detached icy masses, acted on by wind and current, are drifted many a league in an equatorial direction until dissolved under the genial influence of a more vertical sun. From the highest point of Whalefish Islands in 69° N., 53° W., at least one thousand icebergs were observed wending their way to the southward, and Markham in his whaling cruise to Baffin's Bay saw fully seven hundred or eight hundred icebergs at one time, some of which were very large. These pests to navigation have been met with in the North Atlantic as early as December; but it may be fairly asserted that they only descend in large numbers to low latitudes in the late spring or early summer months. One clear day in January, 1874, the ordinary route by the Flemish Cap was obstructed by an immense icefield to south, north, and east, as far as could be seen from a ship's masthead, and shortly afterward ships were observed there with sails furled and frozen in firmly. The Maria ran into an iceberg on December 15, 1842, and the Wabeno collided with an ice island on December 5, 1868, when she was so seriously damaged that her crew were compelled to abandon her. In the latter part of January, 1844, the Virginia was thirty-four hours fast in the ice, and on January 28, 1890, the steamship Colina was similarly situated for twelve hours, with engines stopped, in 48° N., 49° W. Next day, about three hundred miles to the southward, the steamship Mineola passed an iceberg seven hundred feet high and one mile in length. The bark Mary, on February 15, 1860, was suddenly beset by icebergs, in 47° N., 43° W. Her bow was smashed, and her crew had barely time to get into her frail boats ere she foundered. For five days these suffering seafarers were without sustenance, and two died from exhaustion before a rescuing vessel hove in sight. On February 8, 1890, the steamship Yorkshire supplied the brigantine Silvia with provisions, oil, and fuel. The latter vessel had been six days in the ice, and was badly chafed. She was then sixty-seven days out on a passage from Figueira to St. Johns. The steamship Lepanto on February 10, 1890, in 43° N., 49° W., passed an iceberg 500 feet high and a furlong in length. Passages have to be made across the North Atlantic in all kinds of weather, and icebergs, drifting deviously, are a menace to record breakers cleaving the thick fog near the Banks of Newfoundland. Masters and officers of ocean greyhounds are fully alive to the awful responsibility thus thrust upon them at times, when, in the words of Spenser,

> Suddenly a grosse fog overspread
> And heaven's cheerfull face enveloped,
> That all things one, and one as nothing was,
> And this great universe seemed one confused mass.

In July, 1861, the steamer Canadian, while going dead slow in thick fog, struck an iceberg which was almost level with the sea surface. In May, 1890, the steamship Parisian in 48° N., 40° W., was going six knots an hour in foggy weather when an iceberg was sighted only forty yards ahead. The engines were rapidly reversed, but she ran on an outlying ledge and luckily slipped off into deep water without damage. A week later in 44° N., 48° W., the Beacon Light was steaming half speed in a fog, when she collided with an iceberg ninety feet high and six hundred feet long. The blow was a glancing one; nevertheless, the danger was imminent and terrible. Falling ice crushed in her forecastle deck, and other pieces that fell into the sea alongside dealt injury to her hull as they rose beneath her. Six days later the Danish steamer Thingvalla, in 47° N., 43° W., struck an iceberg twenty feet high and one thousand feet long. Her stem was crushed and sixteen bowplates cut down to within two feet of the water line. Much ice fell on her deck and she was detained for sixteen hours making the necessary repairs. A few weeks ago the French steamship La Touraine

narrowly escaped destruction. A dense fog lifted for a few moments and a huge iceberg was reported as right ahead in just sufficient time to steer clear of it. In July, 1890, an iceberg broke in three pieces at the instant a vessel named the Portia was close at hand. One piece came up directly beneath her and she was lifted five feet thereby, strained slightly, and had her rudder twisted. Icebergs were especially numerous in 1890, and the master of a German steamer, the Slavonia, reported to the United States hydrographer that she passed the last remnants of an iceberg on July 10 in 49° N., 24° W. This is probably the nearest approach of icebergs toward our shores since the glacial period. It is thought by some ocean travelers that the proximity of icebergs can be readily detected by frequent observations of the sea surface temperature. We read in one popular work that icebergs "cool the water sensibly to the distance of forty or fifty miles around, the thermometer sinking seventeen or eighteen degrees in their neighborhood," and again, "they cool the water to the distance of thirty or forty miles, and the air to a much greater distance." Another well known writer on physical geography says that the presence of icebergs "can very generally be told in the Atlantic by steady observations with the thermometer." The testimony of practical navigators, however, is almost unanimously opposed to this view. A cold current of water may be bringing icebergs along with it, but the drifting icy masses are not the cause of the low temperature of the medium in which they float. As a matter of fact, it has often been demonstrated that there is not any appreciable difference in sea surface temperature even when close to the largest of icebergs; and this is not very remarkable, having regard to the poor conductivity for heat possessed by water. Moreover, sea temperature could not possibly be an infallible guide, for icebergs have been observed not only in the warmer water of the Gulf Stream, but also to the southward thereof, having been transported thither by a strong undercurrent. A good lookout man may perchance hear the echo of a steam whistle, or the wash of waves, or see the ice blink, but there is no doubt that a shipmaster breathes more freely when his good ship has passed the limits of ice laid down on his pilot chart for the current month.

At the British Association meeting of 1842, it was asserted that unusual prevalence of ice in the North Atlantic was probably the cause of the cool summers over our islands of the four immediately preceding years, and it was urged that fifty years previously icebergs did not usually make their appearance in the North Atlantic until autumn. Professor Archibald, not long since, put forward a similar suggestion; but it would probably be easy of disproof by an unbiased examination of the records during the past few years. The ice season is rather late this year, but during the months of May and June the authorities at Washington received reports of icebergs from two hundred and fifty ships. Captain P. Myrick, the lightkeeper at Cape Race, N. F., reports that ice was exceptionally scarce off that coast during the first three months of the year. He has not known such an absence of all kinds of ice during the last eighteen years, while, at the same time, lakes and ponds of the island were as free from ice as in July.

On May 3, the Norwegian bark Europa passed thirty-seven icebergs between 49° N., 43° W. and 48° N., 48° W., and the master believed that there were doubtless many more in the vicinity, as the weather was too thick to see very far. Next day, the German steamer Stubbenhuk passed sixty bergs in almost the same geographical position. Two days later, the Lake Winnipeg, when in 47° N., 46° W., passed twenty-five large bergs in thirty miles. A week later, the Manhattan passed an iceberg four hundred feet high and one quarter of a mile in length. On the 15th, the master of the Ulunda saw hundreds of bergs of all sizes within two hundred and twenty miles of St. Johns, N. F., and next day a berg was seen aground about half a mile from the entrance to that harbor. The German steamer Hafis, in 46° N., 47° W., passed a large berg which was six hundred feet high and four miles long! In 50° N., 45° W., the sailing ship Habitant spent two days picking her way through a group of twenty-five bergs, some of which were of gigantic proportions. The bark Zimi, on June 6, was beating to the westward in 46° N., 53° W., and every time she stood to the northward it was noticed that the northern edge of the Banks of Newfoundland was crowded with icebergs which were at first mistaken for land covered with snow. The steamer Prodans, on June 13, passed a berg five hundred feet high, and square shaped, with a side of one mile!

Ice is also a danger to vessels making southern passages to the antipodes or rounding Cape Horn. Icebergs from the Antarctic ice barrier are more numerous and more terrible than those from tortuous Arctic glaciers, and drift much nearer to the equator. Being tabular in form, they differ from the fantastically shaped bergs of northern latitudes, and it has been estimated that thirty years' fog and snow are required to complete the formation of the usual southern berg. The Antarctic ice cap has sufficient seaward slope to permit of a forward movement, and the southern icebergs are enormous masses broken off its outer edge, which forms an icy cliff, with an upper surface glistening like a plain of frosted silver.

Vessels arriving recently at Australia from England report having fallen in with many icebergs to the southeastward of the Cape of Good Hope. On 12th December, 1891, in 44° S., 14° E., the Winifred was surrounded by bergs 400 ft. high, one of the largest of which broke up, but not near enough to injure her. About the same date the Salamanca passed ten, and the Persian Empire fourteen bergs. Along the Cape Horn route, however, icebergs have been exceptionally immense and frequent. From the 4th to the 9th February, between 57° S., 147° W. and 59° S., 109° W., the steamship Fifeshire passed very many icebergs at short intervals. But little more was heard of southern ice until a few weeks ago. The splendid Aberdeen iron ship Cromdale, Captain E. H. Andrew, left Australia on 1st March, and all went well till the 6th April, when running nine knots an hour in 46° S., 36° W., a large iceberg was observed right ahead, but only just in time to steer clear of it. At daylight the sea was found to be studded with icebergs. In the words of the "Ancient Mariner:"

> "The ice was here, the ice was there,
> The ice was all around;
> It cracked and growled and roared and howled
> Like noises in a swound."

Sail was reduced, and all hands kept on deck in readiness for any emergency. The Cromdale was standing into an extensive horseshoe-shaped bay in the side of an ice island of almost unparalleled magnitude. Vertical icy cliffs, fully 300 ft. high, were on each side and ahead, with the sullen sea pouring over them in cascades of salt spray. The good ship was extricated from her perilous position by dint of sterling seamanship, but at one time it seemed that she must inevitably be dashed to pieces, with all hands, on the terrible icy shore under her lee. At least fifty of the many bergs in this bay were peculiarly opaque, and Captain Andrew suggests they had just previously floated with the then upper parts submerged. The sea temperature was 51°, and such relatively warm water would rapidly dissolve the under parts of the bergs, and cause them to topple over frequently. The sea surface temperature had been carefully taken at regular intervals; but, as usual, afforded no indication of the proximity of ice, and this observant shipmaster had similar experience three years ago in the ship Derwent. Water within half a mile of a berg showed no change in temperature. One berg passed by the Cromdale was 1,000 ft. high! This may seem an incredible altitude to dwellers on shore, remembering that only about an eighth of a floating icy mass is exposed to view. Nevertheless, we shall show that other instances of equally lofty ice islands are not far to seek. The bark Noel, of Arendal, Capt. Jorgenson, passed an ice island 1,000 ft. high, and nearly 7,000 ft. long, when in 50° S., 118° W., on the 6th October, 1890. On the 16th of last May the Aberdeen bark Strathdon, Captain A. T. Wills, encountered foggy weather, and, upon slightly clearing after two days, in 45° S., 25° W., enormous icebergs were discovered in every direction. Fog shut down again, and it was only by skillful handling that she escaped total loss. One ice island, not less than 1,000 ft. high, was only narrowly cleared. That afternoon she was surrounded by bergs, and the only opening scarcely appeared wide enough for her to pass through. She reached open water by this outlet, but her yards almost grazed the icy heads on each side of her. The weather cleared somewhat and revealed an immense mass of ice, along which she sailed twenty miles to the southwestward, and hove to for the night. At daylight an ice island, having an altitude of 800 or 1,000 ft., stretched along to leeward as far as could be seen, and, after sailing forty miles along it, she got into a clear space and hove to. Next day there were seventy other bergs in sight, but the monster of the previous day had disappeared. The last ice was seen in 42° S., 30° W., on the 22d. A pleasing and almost unique incident in connection with this danger from ice occurred after the Strathdon arrived at London. Captain Wills was presented by his crew with a testimonial expressing their unanimous thanks "in recognition and admiration of the able and seamanlike manner" in which he "handled the ship under circumstances which it has seldom been the misfortune of any merchant vessel and her crew to pass through." Captain Lewis, of the bark Valdivia, reported on arrival at Falmouth, from Pisagua, that on 21st April, during foggy weather, in 50° S., 50° W., he saw many icebergs. When in 43° S., 35° W., at three o'clock on the morning of the 24th, the Valdivia passed a very large berg quite close enough to jump upon it. She was traveling eight knots an hour at that time, but owing to the darkness the berg could not be seen further than a ship's length astern, although when the moon rose it could be distinguished when four miles distant. During the next eight hours she passed eighty-five bergs, some of which could scarcely be discerned until close alongside. They looked excessively opaque from afar off, but nearer proved to be of a dark green color. The last berg, seen at a distance of ten miles, was fully 8,000 ft. long and several hundred feet high. Captain Lewis remarks that the sea surface temperature varied but little on either side of the bergs for some distance. At nine o'clock, on the night of the 27th, in 39° S., 31° W., the bark passed a detached piece of ice 100 ft. long, which was first observed by the noise of the sea washing over it. A large number of bergs had been seen that day, and the last was 130 miles nearer the equator than the limit laid down on the chart. Captain Lewis had experienced an exceptionally strong current to the northeast for some days, and infers that this accounts for the ice being seen so far to the northward. About one o'clock on the morning of 23d April, in very dark rainy weather, the Dundee ship Duntrune, bound from Iquique to Dunkirk, when sailing about ten knots an hour, collided with an ice island in 47° S., 38° W., which was not observed till too late. She rebounded and again struck. Her bowsprit broke off, her bows were smashed in, her foretopmast came down, and the sea swept her decks and poured into the forecastle through the apertures in her bow plates. All hands cleared away the wreck with the indomitable pluck and ready resource of true seamen, while at the same time the boats were prepared, lest worse befall them. At daybreak the Duntrune lay to windward of an ice island 900 ft. high, which stretched northeast and southwest from horizon to horizon apparently without a break. Her position was far from desirable. Four feet of ice lay on her forecastle deck, the gale shrieked across her rigging, and at any moment another collision might take place with the drifting danger to leeward. Eventually she was steered through a narrow opening and found herself surrounded by hundreds of other bergs, with barely two miles of open water. A channel was discovered through this icy circular barrier, and she had only just cleared it when a huge iceberg broke up, effectually blocking up the passage to safety. Had she been delayed for ten minutes her loss would have gone to swell the total of inexplicable shipping disappearances. She passed the last ice on the 26th, and the first news of her misfortune reached England by the French steamship Ville de Rosario, to which she had signaled near the equator. The Duntrune eventually arrived at her destination without further mishap. On 10th May, between 45° S., 37° W. and 43° S., 35° W., the ship Invercargill passed more than 100 icebergs. One was 300 ft. high, and over a mile in length. Many of the others were of equal altitude. The Hesperus, when in 44° S., 39° W., on 18th May, passed a white berg, in shape a square of one mile side; and its height, measured by sextant, 400 ft. Another berg was brown and black, giving it the exact appearance of an earthy island. Captain Ross, the Antarctic explorer, many years ago saw a southern berg with large pieces of rock upon it weighing many tons, and nearly covered with mud and stones. Specimens of the rock were procured, and proved to be of volcanic origin. Another berg, somewhat similarly laden, was deemed an island, and the delusion only dispelled by landing thereon. In the early morning of the 21st May, the four-masted ship Loch Torridon, Captain Pattmann, sighted an iceberg in 45° S., 34° W., and by noon the whole sea as seen from aloft seemed strewn with icebergs all around, which were 300 ft. high and 400 ft. long. On 2d June, in 45° S., 37° W., the County of Edinburgh passed numerous bergs extending 100 miles and ranging from 200 to 900 ft. high.

It will be seen that the southern icebergs are of unusual proportions and moving in close array this year, but we should be wrong to assume that they have broken the record. Icebergs of the southern hemisphere have not infrequently been reported of heights surpassing the limits of belief. An immense icy mass was passed by twenty-one ships between December, 1854, and April, 1855, drifting about in 40° S., 20° W. It was 300 ft. high, 60 miles long, and shaped like a horseshoe, presenting to unwary navigators a sheltered bay between its arms not less than forty miles across. The steamship Great Britain steamed fifty miles along the outer side of one arm; and an emigrant ship, the Guiding Star, having sailed into this icy bay, was lost with all hands. Two other ships, the Cambridge and the Salem, are known to have also been embayed, but fortunately were extricated without damage. A similar, but smaller berg, was once met by the Agra in the North Atlantic. She ran into a bay in the center of an iceberg which was nearly two miles across at the entrance, and experienced great difficulty in beating out again. In August, 1840, the Dutch ship Generaal Baron von Geen, in 38° S., 15° E., passed an ice island 1,000 ft. high, and the Agneta, in March, 1855, passed one 900 ft. high, in 53° S., 15° E. In May, 1887, the bark Inchgreen passed close alongside of an iceberg in the lone southern ocean, which was estimated by Captain Miller, her commander, to have had an altitude of 700 ft. above the sea surface, and to have been quite seven miles in length. In December, 1856, a vessel passed four large bergs close together in 50° S., 43° W., one of which was 700 ft. high. Even when southern icebergs are less lofty than those above quoted, they far exceed the northern icebergs in their other dimensions. In September, 1865, the Alice Davies, in 47° S., 39° W., passed a flat-topped ice island, which was about twenty-five miles long in a north and south direction, but extended to the northwest further than the eye could perceive. The Edmond, Captain Smithers, in December, 1859, passed a berg 580 ft. high, in 51° S., 44° W., which so much resembled a rocky island that it was reported as such. About a fortnight earlier the Gleaner had passed a berg 200 ft. high when one degree further to the westward, which was equally difficult to distinguish from land until she sailed close to it. In December, 1861, the Queen of Nations fell in with a berg in 57° S., 146° W., which appeared to have much earth adhering to it, and in 54° S., 170° W., she passed a larger one, not less than 720 ft. high. In November, 1854, the Great Britain passed no fewer than 280 bergs while running along the 56th parallel of south latitude between the 112th and 92d meridians of west longitude, while at the same time the Golden Era, proceeding along the 63d parallel, did not meet ice until the 72d meridian was reached, when she was surrounded by ice, and narrowly escaped destruction.

Other well authenticated instances might be given of immense icebergs, or ice islands as they are termed indifferently by navigators, having been passed by ships in high southern latitudes, but we have written sufficient to demonstrate that an altitude of 1,000 ft. above the sea surface, as reported by Captain Andrew, of the Cromdale, is quite within the limits of probability for southern icebergs, even after making due allowance for the chance of error naturally present at a time when astonishment and imagination most powerfully affect an anxious observer.

From information set forth on the more recent issues of the weekly United States Hydrographical Bulletin, we gather that some of the passenger steamships have crossed the Atlantic between the Old World and the New without any ice having been seen by those on board. It does not, however, follow that icebergs were not nigh in the vicinity of the Banks of Newfoundland in foggy weather.—*Nautical Magazine.*

HOW OLD IS THE WORLD?

By the Rev. H. N. Hutchinson, B.A., F.G.S.

Geology is not one of the exact sciences with a mathematical basis, like chemistry or electricity. Nevertheless, problems arise now and then which are capable of mathematical investigation. The problem of the earth's antiquity, or rather that of the duration of *geological time*, which is not the same thing, is one that has attracted much attention, and has led to a long controversy between certain physicists on the one hand and geologists on the other. According to the "nebular hypothesis" now generally accepted, our planet had cooled down from a molten and somewhat viscous state long before geological time began—that is, before a watery ocean settled down by condensation from a heated atmosphere, and left our air as it now is, mostly composed of the incombustible element nitrogen, with a little oxygen, a variable amount of aqueous vapor, and a trace of carbonic acid. How many æons passed away before this state of things was arrived at, no one can say. Such times were pre-geological. But at last an ocean formed, then, perhaps, later on, dry land appeared; the wind blew and the rains fell, as they do now, and the earth reached a phase which geologists believe to have been, generally speaking, not very unlike that of the present day. The question of geological time is the question of the duration of this phase. The great series of stratified rocks (including lava flows and intrusive igneous rocks such as "dikes") were formed during geological time; and these are the pages on which the earth has recorded her history. Naturally, therefore, the geologist endeavors to seek for some means of calculating the length of time required by Mother Earth to write her autobiography.

Now the earlier modern geologists, Hutton and his followers, who, by teaching the great principle of uniformity in geological actions, placed the science on a sound and reasonable basis, and gave it an enormous impetus, were, unfortunately, so greatly impressed with this idea that they could see no trace of a beginning or sign of an end. Sir Archibald Geikie, in his recent address as president of the British Association, assembled in Edinburgh, has thus eloquently described their state of mind: "When the curtain was then first raised that had veiled the history of the earth, and men, looking beyond the brief span within which they had supposed that history to have been transacted,* beheld the records of a long vista of ages, stretching far away into a dim, illimitable past, the prospect vividly impressed their imagination. Thus the idea arose and gained universal acceptance, that, just as no boundary could be set to the astronomer in his free range through space, so the whole of bygone eternity lay open to the requirements of the geologist. . . . This doctrine was naturally espoused with warmth by the extreme uniformitarian school, which required an unlimited duration of time for the accomplishment of such slow and quiet cycles of change as they conceived to be alone recognizable in the records of the earth's past history."

This extreme teaching, in itself a reaction against the old-fashioned previous teaching, produced another reaction, and the pendulum of opinion swung back to some extent; only slightly, but still sufficiently to raise a controversy. The physicists, led by Lord Kelvin (Sir William Thomson), began to look about for some means of checking these enormous demands. Lord Kelvin considered the question of the world's antiquity from the physical standpoint. His arguments, or rather calculations, were based on three important considerations. These we must notice; but as our object in this paper is to consider purely *geological* measures of time, and his methods can only be judged by the mathematician and astronomer, we must content ourselves with a very brief account of his conclusions. Lord Kelvin arrived at a very different conclusion, and this was derived from three distinct lines of reasoning, or rather calculation. *First*, he considered the internal heat, and rate of cooling of the earth; *secondly*, the tidal retardation of the earth's rotation; and *thirdly*, the origin and age of the sun's heat.

With regard to the earth's heat: the rate of increase of temperature downward from the surface is known, for a certain distance, by observations in mines. As many of our readers are already aware, it is about 1° F. for every 50 or 60 feet. But this rate is not maintained, and becomes less at great depths. Then with regard to the earth's present temperature—about 36° F. at the bottom of the oceans. From such available data he calculated that the earth could not have consolidated, from its former molten state, *less* than 20 millions of years ago, nor *more* than 400 millions. In the one case the underground heat would have been greater than it actually is; in the other there would have been no sensible increase in temperature downward. He afterward inclined toward the lower limit rather than the higher one, and said that we ought to be quite satisfied with 100 millions of years for the duration of geological time. Professor Tait would even limit the period since the earth's consolidation to 10 or 15 millions of years.

We pass on to the argument from the tides. It is generally admitted that the daily tidal waves must, in some degree, diminish the rate of rotation of the earth on its axis. Its action has been compared to that of a brake on a wheel. At one time, then, the rotation was more rapid; in other words, the earth's day was shorter, and has since been steadily getting longer. If we assume any antiquity for the globe greater than 100 millions of years, he thinks the flattening at the poles would be greater, owing to greater centrifugal force having been formerly exerted by the more rapid rotation.

Lastly, Lord Kelvin† has attempted calculations based upon the radiation of heat from the sun, and also upon the amount of heat generated by the falling together of meteoric masses, such as by clashing together may have given rise to the sun. He admits, however, that his conclusions from this source are, from the nature of the case, less reliable. Still, like the other calculations, they point to a comparatively small number of millions of years, perhaps about twenty. The sun may, however, have continued to receive showers of meteorites, and thus to be replenished with heat; which would disturb these calculations. Moreover, certain chemical changes may be the means of liberating heat in the sun.

But we will not dwell on these difficulties here. It is hardly necessary to say that most geologists consider that conclusions such as these are too sweeping. Seeing what vast changes have taken place on the earth—so many thousands of feet of solid rock formed by slow deposition in water, so many new forms of life introduced at certain epochs, while others were extinguished—the geologist cannot bring himself to believe that all the changes (only fully realized by those who study the record of the rocks) could have taken place within 20 or even 100 millions of years. Some, doubtless, would demand much more time, and refuse to accept even the limit of 400 millions. No one distrusts the actual calculations; but many do seriously distrust the data (or want of data) upon which they are founded. Hence a serious difference has arisen between geologists and physicists with regard to the duration of geological time. Mathematics are an excellent mill, and will grind out beautiful results; but what you get out of this mill depends very much on what you put into it, and if you put in material based upon certain assumptions, you must not be surprised at getting a result tainted with similar uncertainty.

Let us quit this somewhat unsatisfactory region of speculation and see what further light can be gained from the science of geology. It will be interesting to compare any results that may be obtained with those above mentioned, and to see whether they harmonize.

The geologist knows only two time-keeping processes; one is *rock formation* (deposition), the other *rock destruction* (denudation). A third is sometimes referred to, namely, changes in the organic world involving the appearance, from time to time, of new species, genera, families and orders of plants and animals—changes which are comprehended under the one word "evolution." But this kind of change, which has been going on ever since the oldest (Archæan) rocks were first formed, concerns the biologist more than the geologist. The biologist, as Professor Huxley said, has no clock, and must take his time from the geological clock. In other words, when, on passing from one rock formation to another, a great change in the fossils is noticed—as, for instance, in passing from primary rocks to secondary or from secondary to tertiary—the lapse of time required to bring about such evolutionary change can only be gauged by the thicknesses of the strata in which the different fossils are found, and partly, in the two cases above quoted, by the "stratigraphical break" between the two sets of strata; that is, the amount of rock denuded during the interval between the two eras. As the Greeks used to detect "the lazy foot of time" by the slow dropping of water from a clepsydra, so the geologist measures his periods by the work of water, either as a rock destroyer or as a rock former. This is our water clock, and our two measures of time are (1) depth of rock denuded, (2) depth of rock deposited. Now the condition of the water clock's accuracy as a time keeper was uniformity of action, that the drops should continue falling at the same rate; so with the geological clock. These two processes, so closely related to each other, must be supposed to have been working throughout geological time (that is, the time during which the great series of stratified rocks were being formed) with considerable uniformity. This brings us back to the "theory of uniformity," originated by the illustrious Hutton, and expanded and explained by Playfair and Lyell.

Readers of *Knowledge* will hardly need to be told that "denudation" is chiefly effected by "rain and rivers." The consequence of "denudation" in one place is rock formation in another; the one is complementary to the other. In other words, the *debris* of continents is carried by rivers into lakes, seas and estuaries, there to settle down and "sow the dust of continents to be." Now rivers depend for their supplies on *rainfall*; hence rainfall is one of the main factors in problems about denudation. Geologists believe (from a mass of evidence in the stratified rocks which it would take too long to expound here) that the rainfall has, in past periods, been pretty much what it is *now* in various parts of the world—not necessarily in *Europe*. It *may*, however, have been somewhat greater as far back as the Archæan and Palæozoic times, when, perhaps, the earth was ostensibly warmer and the sun sensibly hotter. Hence, geologists consider that they are justified in attempting to form some kind of estimate of former periods of time from the two processes above referred to. Not only is it possible thus to compare one period with another and to say which was the longest, but we venture to think that it is justifiable to attempt to calculate the limits of geological time on the basis of the rate at which strata may be formed. We want to translate feet of rock formed into years. To give a mathematical basis to geology is one of the great problems of the future. What degree of success awaits such efforts we cannot say, but certain attempts have been made to gauge denudation, and to see at what rate it goes on. With regard to deposition of strata, very little, if anything, has been done, and we cannot help thinking that important results might be obtained in this direction; but of that we shall speak presently.

Let us briefly consider the first operation, namely, the wearing away of land. The subject of atmospheric denudation has been arithmetically investigated, in order to ascertain at what rate a given continent, or portion of a continent, is at present being worn down by "rain and rivers." Take the great area drained by the Mississippi, which is what geographers call its "basin." The area of this basin is reckoned to be 1,147,000 square miles. It is clear that all the mud, sand, etc., brought down by this great river to the Gulf of Mexico must be derived from the rocks and soil in that area; the next step is to find out how much solid matter is brought down every year. Most extensive and accurate determinations upon this subject have been made by the United States government. As the mean of many observations carried on continuously at different parts of the river for months together, Messrs. Humphreys and Abbot, the engineers employed to investigate the physics and hydraulics of the Mississippi, found that the average proportion of sediment contained in the water is $\frac{1}{1755}$ by weight, or $\frac{1}{3070}$ by volume. But, besides the matter held in suspension, they observed that a large amount of coarse detritus is constantly being pushed along the bottom of the river. They estimated that this moving stratum carries every year into the Gulf of Mexico about 750,000,000 cubic feet of sand, earth, and gravel. Their observations led them to conclude that the annual discharge of water by the Mississippi is 19,500,000,000,000 cubic feet, and consequently that the weight of mud annually carried into the sea by this river must reach the sum of 812,500,000,000 pounds. Then, taking the total annual contributions of solid matter, whether in suspension or moving along the bottom, they found them to equal a prism 268 feet high, with a base of one square mile.

But, besides all this, there is in every river a large amount of matter chemically dissolved. This consists chiefly of carbonate of lime, dissolved by rain water in filtering through rocks before it reaches the river. Properly to estimate the loss sustained by the surface of a river basin, we ought to know the amount of mineral matter thus removed, as well as that referred to above; and to make sure of good results, we ought to have the total volume of water discharged, from measurements made at different seasons *and* extending over a series of years. Such data have not been fully collected from any river, though some of them have been ascertained with approximate accuracy, as in the cases of the survey of the Mississippi and the Danube. As a rule, more attention has been paid to the amount of mechanically suspended matter than to the amount in solution.

We must, therefore, confine ourselves to the former, but it must be borne in mind that the following estimates are under-statements of the truth, because the amount of dissolved matter is left out. Some of the results obtained are as follows: The Mississippi, with a basin of 1,147,000 square miles, discharges annually 7,459,267,200 cubic feet of solid matter; the Rhone, with a basin of 25,000 square miles, discharges 600,381,800 cubic feet of solid matter; the Danube, with a basin of 234,000 square miles, discharges 1,253,738,600 cubic feet; the Po, with a basin of 30,000 square miles, discharges 1,510,137,000 cubic feet. Now as all this solid matter comes off the surface of so much land, the area of which is known, it can easily be calculated what thickness of rock must have been removed (on an average) to produce the amount brought down to the sea, as given in cubic feet.

On elevated land, where mountain streams run faster, more rock is removed than over low plains or gentle slopes, where rivers run slowly. But we only want a general average for the whole area. An illustration may serve to make this clear. Given a lump of butter, containing so many cubic inches, and a slice of bread, with area so many square inches; any school boy could find what the thickness of the butter would be when spread evenly over the bread. The results for the great rivers were as follow: The Mississippi removes $\frac{1}{1320}$ foot from its area in one year, or one foot in 6,000 years; the Rhone removes $\frac{1}{1728}$, or one foot in 1,528 years; the Danube removes $\frac{1}{1873}$, or one foot in 6,846 years; the Po removes $\frac{1}{730}$, or one foot in 729 years. Now these are very important results, and since the physics of the Mississippi have been more carefully studied than those of perhaps any other river, and as that river drains so extensive a region, embracing so many varieties of climate, rock and soil, we shall probably get the best results by taking the Mississippi rate of denudation as a fair one.

Let us see, then, what that rate means. It means that the surface of its basin will be lowered 10 feet (generally) in 60,000 years; supposing the rate to continue, 100 feet in 600,000 years, and 1,000 feet in 6,000,000 years. Apply this to the whole of North America, the mean height of which, according to Humboldt, is 748 feet above sea level, and we find that this continent would be worn away in about 4½ millions of years. The same kind of calculation, based upon the rate of denudation by the Upper Ganges, has been applied to the continent of Asia, and a shorter length of time was found to be required to wear it all down to sea level. But the Ganges rate seems to be hardly a fair one, so we will keep to the Mississippi. Such calculations are made on the assumption that no serious changes take place in the way of earth movements, raising or depressing a continent. Upheaval would undoubtedly quicken the rate of denudation, by giving greater velocity to the rivers (on account of increased fall), and in the same way depression would check the rate of denudation. But in spite of this possible element of disturbance, the result above given is an important one.

Now the amount of denudation that might thus take place over the North American continent is *a mere trifle* compared to the vast denudation which *must have taken place* in order to provide the prodigious amount of solid matter contained in the whole series of stratified rocks. Their total estimated thickness is about 100,000 feet! It is clear, then, that a much greater number of millions of years was required to lay down this great series of sedimentary rocks on ocean beds, especially when we reflect that such material had to be distributed by ocean currents over vast areas, and also that many of these rocks were built up very slowly in the deeper parts of oceans by the slow accumulation of organic remains. This applies, for example, to the carboniferous limestone, the oolites, and the chalk formation.

Evidences of great denudation abound both in Great Britain and in Europe, and in all parts of the world. Thousands and thousands of feet of solid rock have been removed, and yet such phenomena were by no means spread over the whole of geological time. We can often prove that even in the interval between two successive periods enormous denudation took place, and the mind is bewildered in endeavoring to combine with any reasonable amount of time required for such intervals the much greater periods required for the accumulation of the subsequent or overlying strata. Any student who is familiar with geological sections can call to mind numerous examples of great denudation.

For instance, what a vast period of time is indicated by the upheaval and subsequent denudation of the pre-Cambrian (or Archæan) rocks before those of the Palæozoic era were deposited on their upturned edges! No attempt has been made to estimate in years this interval. Or, to take another case, it is found that in many parts of our country a great thickness of the carboniferous rocks, especially the coal measures, was denuded away *before* the advent of the Secondary or Mesozoic era. Sir Andrew Ramsey has calculated (from sections drawn to scale) that a covering of rock to the depth of one mile was removed from the surface of the Mendip Hills, and most of this destruction took place during the above interval. No one has yet attempted to apply a rate of denudation to this case, for the uncertain elements in such a problem are many. The Mississippi rate of one foot in 6,000 years would hardly be applicable, being an average for a large area including mountains, valleys, and plains; whereas the Mendip Hills are a small hilly area. If we could find the rate at which some of our present mountainous regions are being worn down, and obtain an average therefrom, it might be justifiable to apply such an average to the case in point. But mountains are composed of hard and often crystalline rocks, and this fact would tend to counteract the more rapid erosion due to the velocity of mountain streams.

We will now endeavor to point out a method that might possibly lead to valuable results if followed, and from which an average rate of rock formation might be obtained.

Take the case of the Mississippi. What becomes of all the solid matter brought down by this river? It mostly finds its way into the Atlantic, for the Gulf of Mexico is swept by that powerful current the Gulf

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* Viz., the 4,000 years of Archbishop Ussher's Chronology, a mere "pious opinion," nowhere expressed in Scripture.

† There is some uncertainty as to the amount of heat radiated by the sun. Assuming Helmholtz's theory as to the source of the sun's heat, that is that it is due to the slow contraction of the sun's bulk, and assuming that the sun has derived its heat solely in this way, we may say with some certainty that the contraction of the sun to its present size from a diameter as large as the earth's orbit would not have furnished more than 20,000,000 times as much heat as the sun now supplies in a year. But from theoretical considerations it seems probable that a gaseous mass is losing heat by radiation, and contracting under its own gravity, must rise in temperature. So that it is possible that the annual loss of heat in former times may have been less than at present, even though the radiating surface was then greater.—A. C. RANYARD.

Stream. It would not be spread *all over* the Atlantic bed, for some may be carried up to the North Sea and Arctic Ocean; and again, there are large areas in the Atlantic where no *sedimentary* deposits are forming, but only globigerina ooze, pteropod ooze, or the red clay (believed to be volcanic and even cosmic dust). These areas are far from land, and some of them are the deepest recesses of the Atlantic. Suppose that all the *debris* from the North American continent were being washed into the Atlantic only. Now this ocean is larger than North America in actual area, but we may subtract the areas devoted to globigerina ooze or red clay.

What these areas are could doubtless be estimated by Mr. Murray, of the Challenger expedition. We do not know how much they are; but let us suppose that when this is done an area remains equal to the continent of North America. Then it would follow that all the rock material removed from that surface of land settles down to form new rocks *on an area equal to that of the land from which it came*. Now if, taking the Mississippi rate, one foot is removed from the former area in 6,000 years, it follows that about one foot is added to the latter surface in the same time. It would really be rather more, because the new material would be soft and unhardened by pressure, while the old rocks from which it came were compressed and hardened before they came up to form a land surface. But this difference may be neglected. It will thus be seen that a result of some value is obtained, namely, just what we have been seeking—an average rate of rock formation.

The question arises, "Is this rate of rock formation over a large area of sea bed, viz., one foot in 6,000 years, too rapid?" We are inclined to think that it is. It might apply to strata formed in shallow waters, but it seems too high a rate for those formed in deeper waters, and certainly is inapplicable to slow-growing deposits like globigerina ooze. However, let us see what we can make of it. The whole series of stratified rocks is generally estimated at 100,000 feet—taking all the formations and adding their thicknesses together. Here would be a measure of geological time, if only we knew the average rate at which they were built up. Suppose we apply the rate just obtained and see what it leads to. If one foot is formed in 6,000 years, 100 feet will be formed in 600,000 years, and 100,000 feet in 600,000,000 years. Six hundred millions of years! This is more than Lord Kelvin's extreme limit for geological time, or the time since the earth consolidated from a molten state. And yet we have taken a rate of rock formation that appears not to err on the side of rapidity; and, moreover, this calculation makes no allowance for those great "gaps" or "breaks" in the 100,000 feet of the geological record with which the student will be familiar.

Again, it makes no allowance for the necessarily slow rate at which organic deposits were formed, and formations of this kind occupy no small fraction of the whole series of rocks. For example, the great mountain limestone in one district is 4,000 feet thick; the chalk in the Isle of Wight is 1,000 feet thick; then there are the oolites between, to say nothing of Silurian limestones below.

It is, therefore, not surprising that geologists are dissatisfied with the limits laid down by Lord Kelvin and others. They demand much more time than he will allow, and we think that the calculation above given justifies such a demand. His later estimate of only 100 millions of years certainly seems too small. Prof. Huxley, some years ago, endeavored as it were to make peace between the two parties in this controversy by taking the latter limit of 100 millions and applying it to the stratified rocks.

If 100,000 feet of rock were formed in 100 millions of years, then the rate of rock formation would be one foot in 996 years—say, roughly, 1,000 years. Now the result we obtained above was one foot in 6,000 years, so that our rate is six times slower than that which follows from Lord Kelvin's computation, and we venture to think that it would be more acceptable to geologists.

One cannot help hoping that before long some attempts will be made to observe the rate of deposition in different seas. Would not observations of the amount of sediment suspended in sea water, taking samples from various depths, be useful? But it would be better still if some one would let down vessels (like rain gauges) on to the bed of the sea in various spots, leave them there for twenty years, and then take them up and measure the amount of solid matter contained in them. They could be attached to buoys by thin wire ropes; thus the sites would be indicated and they could be pulled up. Or again, perhaps in the future an international committee of scientific men may be formed to observe and measure the amounts of *debris* brought down to the Mediterranean by all the principal rivers flowing into it! It would take a long time, but the work could be divided up, and when done we should have a fair idea of the amount of sediment settling down in that area of sea, and so could calculate the rate of rock formation that obtains there.—*Knowledge.*

A FLORAL CLOCK.

AMONG the many persons who are daily attracted to the Champ de Mars by the various exhibitions that succeed each other there from the opening of the season, there are very few who suspect that, at a few steps from there, there is installed a masterpiece of mechanics, which is at the same time a model of decoration for gardens. We refer to the floral clock exhibited at the lower end of the Trocadero, at the corner of Le Notre Street and Quay Debilly. Let us mention, by the way, the curious coincidence that, in the installation of this clock, has caused the selection of a street whose name recalls the celebrated artist who, so to speak, created the art of garden decoration.

The dial (Fig. 1), which is no less than 30 ft. in diameter, is formed entirely of various plants selected from among such as are used in what, in the language of gardening, is called *mosaiculture*. The hands that move over this dial are covered in the same way, so that the eye is not offended by the sight of any of the parts of the mechanism. The latter, which is located in a chamber beneath the dial, as shown in Fig. 2, consists of a central vertical shaft to the upper extremity of which is fixed the frame of the minute hand. This shaft passes into a cylinder upon which is fixed the hour hand. The two shafts are connected by a system of gearings in a ratio such that the minute shaft makes twelve revolutions while that of the hours is making one.

It is the minute shaft that supports the entire weight of the hands. The shaft of the minute hand is capable of easily revolving in that of the hours, which supports it through the intermedium of a small train of rollers

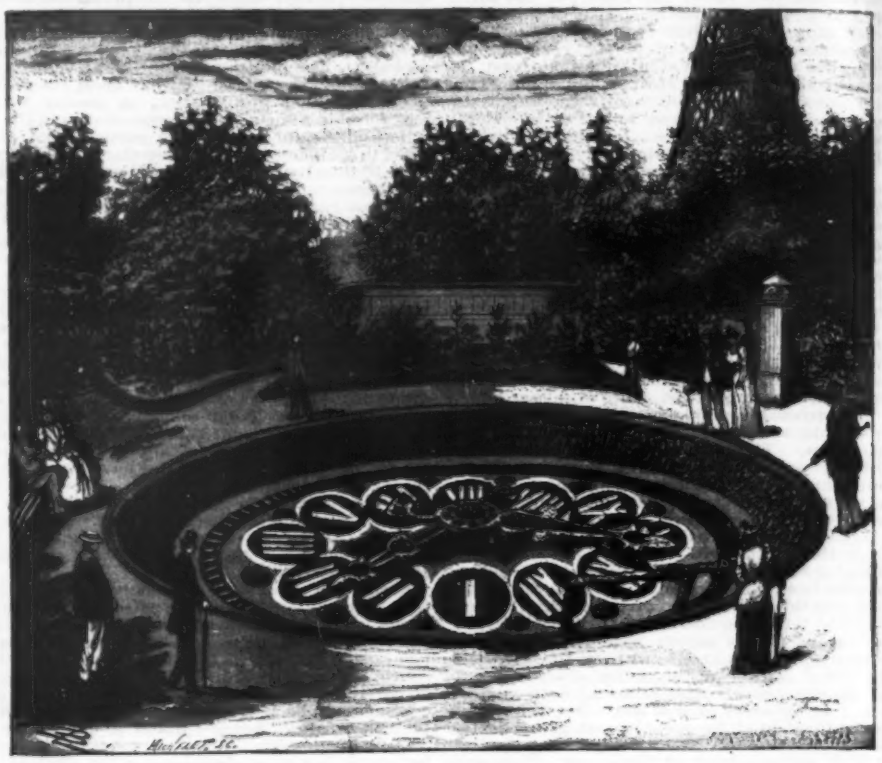


FIG. 1.—THE FLORAL CLOCK.

that converts the sliding friction into rolling friction. Beneath this train of rollers there is keyed a conical pinion which receives its motion from a horizontal shaft upon which acts the motor, which is water. The latter arrives through an abundantly supplied cascade, from the basin of which it runs into a reservoir provided with a waste pipe, whence the excess escapes to the sewer. The object of this waste pipe is to keep up a constant level in the reservoir, for it is thence that flows the water that actuates the system, and it is necessary that the flow shall take place under a constant head.

As may be seen from the figure, the water thus enters a tube provided with a funnel that leads it to a distributer which empties it alternately into one of the two reservoirs, A and B, placed at the lower part and in which are placed floats. If we consider one of them, we shall see that, in measure as the water enters, the float, P, rises and the rack, E, with which it is provided acts upon a toothed wheel, G, which, through a click, L, carries along a ratchet, O, keyed upon the horizontal shaft and giving motion thereto. When the float has reached the upper extremity of its travel, and the reservoir is full, a gearing effects the reversal of a key which controls a two-way cock fixed in the pipe. The water ceases to enter this first reservoir and flows into the second. At the same time, an identical arrangement permits the water of the first reservoir to flow to the sewer. Consequently, the float descends and carries along the toothed wheel fixed upon the horizontal shaft, and, after this, has no action upon the operation of the apparatus. As soon as the second reservoir is full, a contrary phenomenon occurs, and so on.

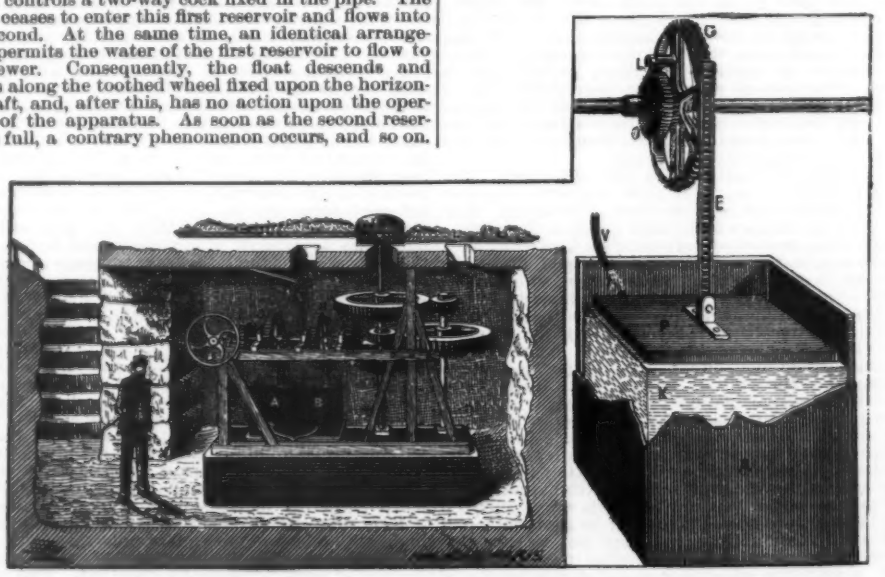


FIG. 2.—DETAILS OF THE MECHANISM.

Upon the minute shaft there is likewise mounted a gearing that actuates a bell placed outside of the dial and that sounds the hours and the halves.

By reason of the dimensions of the floats and the weight of the parts, the motion is not entirely continuous. The float rises only about every thirty seconds, when a certain volume of water has entered the reservoir. The minute hand therefore moves forward by jerks, precisely as in pneumatic and other similar clocks.

Further, in order to overcome the passive resistances, which are enormous in an apparatus of so large dimensions, recourse has had to be had to a windlass, around which winds a cord provided with a counterpoise that is wound up from time to time.

Placed upon slightly sloping ground, as are in most cases the groups of flowers in parks and gardens, such a clock would produce a very pleasing effect from the standpoint of ornamentation. Moreover, much smaller models than the one just described, having a diameter of 3 or 6 ft., may be made, and may be easily placed in a hot house and have the advantage of costing but little and of requiring but a small supply of water.—*Les Inventions Nouvelles.*

THE DAISY PEA.

SOME weeks since we formed part of a party invited by Messrs. James Carter & Co. to see a field of peas in growth. Many have seen illustrations of rows of pea haulms propped up and supported by the ripening pods, and many have smiled skeptically at what they considered the playful imagination of the artist. A visit to Messrs. Carter's field of Daisy peas sufficed to show that, had the artist been present at Bromley, he would not have to tax his imagination much. There were about 3 acres under growth in rows 3 ft. apart, and throughout the crop was uniformly large. The Daisy pea is a dwarf wrinkled marrow coming in after the early varieties. Its haulm is about 18 in. in height, or a little less, copiously furnished with symmetrical, well filled pods, about 4 in. in length, each with nine or ten peas.

The history of the Daisy pea is as follows: With the object of filling the somewhat bladder-formed pod of Culverwell's Giant Marrow, Messrs. Carter crossed that variety with Stratagem, the result giving them a distinct-looking pea. In the following year they again

crossed the produce of the first cross with Stratagem, the result, after another year or two of careful selection, being the present Daisy pea.

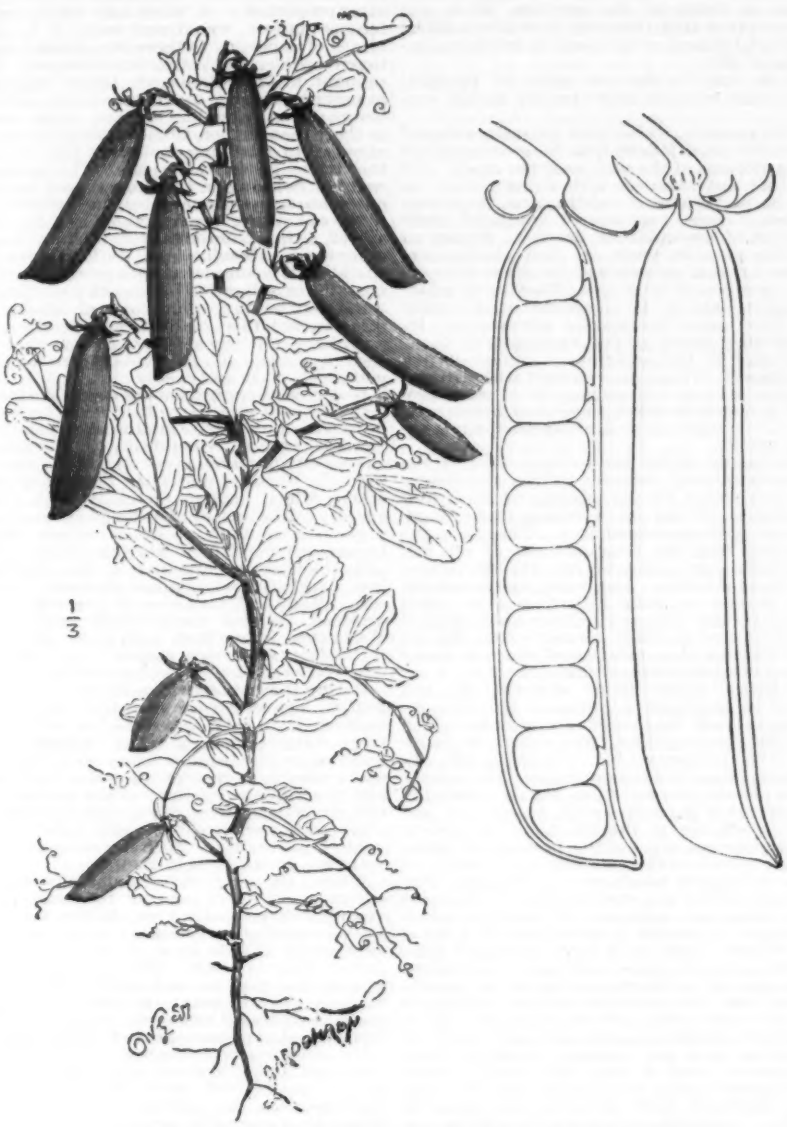
The flavor of the pea is excellent, and from its great productiveness it is likely to attract the attention of market growers. As seen at Bromley, it was grown without support of any kind, but we think it would pay to stretch a wire along the rows, and insure the exposure to light and air of all the pods, many of which, as we saw them, were buried beneath the others, and calculated to invite the attentions of mildew, had such been present, as was happily not the case. The Daisy pea has received an award at the hands of the fruit committee, and our representation of the plant gives a good idea of its appearance.

Messrs. Carter showed us on the same occasion various other crosses among peas, one of which was raised between Daisy and Lightning, supposed to be the earliest white pea at present in cultivation. The object in this case was to get a better flavor in the early pea; but one interesting and peculiar feature in this case was the greater height that the cross-bred pea produced when compared with that of the other parents, as the Daisy may be measured at 18 in. in the garden and 9 in. in the field; while the Lightning may be measured at 2¼ ft. in the garden and say 18 in. to 2 ft. in the field. There is no advantage in this length of straw; in fact, Messrs. Carter should have preferred it if the straw had not been so long.

As illustrating the variety produced by this process of crossing, and the consequent time, care, and labor required in selecting and "fixing" any particular characteristic that may be required, and of eliminating others that are undesirable, we give an extract from our note book relating to nine pods of one cross, taken at random. It shows, what, indeed, every gardener knew before, that the adage, as like as peas in a pod, is one which is much less true than the unobservant imagine. In one pod containing eight seeds, we found five blue and three white seeds, all wrinkled. In one case there were two pods on one stalk, of which one had one blue and five white seeds, all wrinkled; the other had one blue and four white seeds, also all wrinkled. In other pods there were respectively eight peas, all white, five round, three wrinkled; seven peas, six round white, one round blue; eight seeds, all blue, seven round and one wrinkled; this was growing on the same stalk as the preceding; eight seeds, all blue, four round and four wrinkled, intermixed; seven seeds, of which two were white wrinkled, one blue smooth, one white smooth, and three abortive; five seeds, of which four were smooth white, one smooth blue; three seeds, two wrinkled white, one wrinkled blue; and lastly, a pod with five seeds, of which four were wrinkled white, one wrinkled blue. Thus, out of eleven pods of the same variety, sixty-seven peas were taken, of which thirty were smooth and thirty-seven wrinkled, thirty-nine were white and twenty-eight blue. Plenty of work yet before anything like fixity of character can be obtained.

We have some recollection that Mr. Laxton several years ago showed specimens before the scientific committee, which went to show that the seed coat, and not only the embryo within it, might be influenced by the pollen of a particular variety.

Verily the changes that may be produced by cross-breeding seem endless. It must be admitted, however, that only an infinitesimal proportion afford any practical advantage to the experimenter over what he had before. To an enthusiast, that circumstance only makes success more sweet when it does come.—*The Gardeners' Chronicle.*



THE DAISY PEA. (Reduced two-thirds; pods real size.)

DEATH.

By P. CHALMERS MITCHELL.

THIS make no pretension of being an elaborately thought-out essay on "Death." It is the result of an attempt to clear up my own ideas on a subject which, when vitalistic conceptions are spreading so rapidly among biologists, seems to be a last outpost of the other school, and yet an outpost which may be the center of its victory.

I propose to offer no historical introduction; for just as nothing so entirely unfits one to understand current political problems as what is called the study of history, so, however interesting to the curious, the past history of any scientific controversy is the most misleading of all guides to its subsequent development.

In biology, as in morals, simplicity of term invariably covers confusion of idea. The apparent opposites, life and death, like the apparent opposites, right and wrong, extend as a vast tangle of intermeshing ideas. As out of a naive acceptance of right and wrong as simple ideas come the cruel follies of the conscientious, so out of a naive acceptance of the simple words life and death as corresponding to simple facts comes the grotesque abstraction—vitalism.

There are three ways in which an organism may come to an end. By some gross accident of life, as when one cleans an amœba off a slide with a duster, or one's self trifles with nitro-glycerine, an organism may be swept out of existence. Or, and specially in the case of the higher organisms, some alteration in the machinery, some slight defect in the mechanism, may interfere fatally with the whole. Or, lastly, the wrought iron itself may slowly oxidize, or by constant vibration become crystalline, the cells and protoplasm of the living organism may decay. Of course, the divisions of this classification, like those of all classifications that do not deal with beans or buttons, or the subject matter of metaphysics, are not mutually exclusive. They represent only the point of view for the moment convenient to the classifier.

With the first mode of death, though it probably enormously preponderates in the animal world—so enormously as to make it doubtful whether natural selection can have anything to do with fixing the term of natural life—we have at present little to do. The individual life has been destroyed by a mechanical force. The potential energy of the organism provides food for luckier organisms, and sooner or later passes from the world as heat. But in the large organism, as in the small, what has taken place is mechanical interruption of a complicated process, resulting ultimately in chemical death—the running off of the potential energy.

The leading idea in this paper is to find in the various phenomena grouped under the term death, never the opposition of a kindred mysterious force to the mysterious vitalism, but the action of known mechanical or chemical agencies, and to suggest the inference that the life of the organism or of the cell, coming to an end as it does by known agencies, cannot itself depend in essence on any transcendental force; that, in fact, as the negative conditions of life are purely chemical and physical, life itself, however complicated, must be a congeries of phenomena ultimately to be resolved into purely chemical and physical forces.

When we consider the higher animals, we find that death is always more or less violent. As Michael Foster says: "The central factor of life is the circulation of the blood, and the blood is not only useless but injurious unless it be duly oxygenated, and in the higher animals the oxygenation of the blood can be duly effected by means of the respiratory muscular mechanism presided over by the respiratory center in the bulb. Thus, the life of a complex animal is, when reduced to a simple form, composed of three factors—the maintenance of the circulation, the access of air to the hæmoglobin of the blood and the functional activity of the respiratory center; and death may come from the arrest of any one of these three. As it has been put, death may begin at the heart, or the lungs, or the brain."

It requires little medical knowledge to refer the final symptoms of any fatal disease to one of these—the three gates of death of the older writers. Turning over a few of them, one finds the proximate causes of death to group themselves simply.

Death through the heart may come by inhibition, as when a blow on the abdomen or iced milk on the stomach stimulates the peripheral ends of the vagus. Or it may come by cardiac failure, as in fatty degeneration, or in valvular diseases.

Death through the lungs may come as asphyxia from lung diseases, from capillary bronchitis, or from pneumonia. Or asphyxia may result from anæmia caused by loss of blood from hæmorrhage or want of hæmoglobin. Or the respiratory nerves may be paralyzed, as in diphtheria or bulbar paralysis.

And, lastly, death through the brain may come in a multitude of ways. Centers may be suddenly destroyed by wounds, or by the culmination of tumors, or they may be paralyzed by high temperature, or by a multitude of organic poisons arising from organic diseases, like Bright's, or produced by parasitic bacteria.

Even in death from old age we find the dark mother throwing open one of the three gates. The frame of a higher animal does not, like the Autocrat's one-horse shay, slowly and equally decay until there is a simultaneous collapse of everything—until each cell is senile and moribund—but the heart or the lungs or some center in the brain suddenly gives way. But here, perhaps, more plainly than in other cases, we see that the violence or suddenness of death, on which so much stress has been laid, is the merest accident, due to the complexity of the organism. There has been going on for long a gradual degeneration of the walls of the blood vessels—a degeneration showing itself by an accumulation of fat or of lime, products usually oxidized or excreted—and this slow cellular change has, by weakening the circulation, acted on all the organs of the body, and reacted on itself until some point is reached at which the gross mechanism breaks down. So in the cases of most causes of death, we can trace back the chain of events until we find that what is said to cause death is only an accidentally last term in a long series of protoplasmic changes; changes, as in lung diseases, where the lung cells have been gradually decaying, or, as in Bright's disease, where kidney cells have been failing to do their work.

In short, protoplasmic changes have been going on, and when the results of these have sufficiently accumulated, the gross mechanism is suddenly arrested; or, more directly, an interference may result when some natural mechanism is accidentally over-stimulated, as when reflex stimulation of the vagus stops the heart, or when a lethal alternating current is switched through the spinal cord; or, again, a poison may paralyze a brain center, and so the heart or the lungs be stopped.

It is clear that in all these cases—the modes of death treated of in the medical books—a confusion of ideas exists. The source of this confusion is clearly seen when one finds, for instance, Foster suggesting that the point of death cannot be determined, as it depends on the skill of the experimenter endeavoring to resuscitate a flickering vitality; or Weismann and Gotte gravely discussing whether there can be death without a corpse, and whether death is a subtile attribute of the metazoa teleologically built up by natural selection into its present form. We have to distinguish between protoplasmic death and the stoppage of a complicated organism.

We know with absolute certainty that whatever be the details of the processes going on in a living cell, the sum of the activity is the acquisition of potential energy either indirectly by the transformation of kinetic energy, as in green plants, or directly by the absorption of chemical compounds possessing high heat equivalents—I mean compounds which, if oxidized in a calorimeter, melt relatively high quantities of ice; the change of these chemical compounds into substances that can melt relatively smaller amounts of ice. The difference of heat equivalent represents the work done by the cell and the waste in doing the work. Protoplasmic death is an arrest of this process, an arrest causing in the first place a diminution or cessation of absorption of fresh food—of more potential; and in the second, an increase in the heat waste, and consequent lowering in the heat equivalents of the discharged products. This lowering may be rapid, as where the cell tissues are rapidly oxidized in the

air, or it may be a slow process, gradually going on in the bodies of animals or tissues of plants. But the essence of protoplasmic death is that the self-feeding gear is disarranged, that the cell either stops taking up more food, or that it uses its food more quickly than it can take it, and so burns itself away.

In a complicated aggregate of cells, protoplasmic death of any one of them must necessarily be reflected on the others in some mechanical way, the simplicity of the path depending on the simplicity of the cell aggregate. The suddenness or violence of the interference will similarly depend on the degree of complexity of the cell aggregate. In an organism like man the arrest of the cell activity of the respiratory center causes what is called sudden death of the entire organism. But obviously the suddenness, as the death itself, depends on the correlations between the organs —correlations of clearly mechanical nature; and the death of the entire organism—protoplasmic death of all the cells—comes only gradually, as the destructive changes set going creep along the mechanical channels. There is, of course, absolutely no reason why the skin of a man killed by chloroform or gunshot should not succeed as a graft quite as well as the skin from a living man, provided it were taken before the ripples of nutritive and other disturbance had spread to it. The tissues die slowly, and the time of death varies as the mechanical relations.

It is clear, then, that death of the organism, the death of the medical books and the popular imagination, is a function, not of the organism but of the complexity of the organism. It forms a mechanical middle term between protoplasmic death of some of the tissues and protoplasmic death of all the tissues. We may distinguish this as somatic death.

Running down the animal kingdom from the more integrated to the less integrated, we find somatic death gradually fading away till, in the protozoa, there is no distinction between somatic death and protoplasmic death. A shock that is fatal to civilized man is not infrequently borne with comparative ease by savage man, and might have little effect even on one of the higher mammalia. Brain operations and accidents instantly fatal to man can be survived for many days by pigeons, and the removal of great parts has little effect on the life of an amphibian.

Precisely the same condition of things can be inferred if we study the results of artificial section, or the conditions of natural budding. Bisection certainly would kill all the higher animals. In its natural form it becomes an increasingly common form of reproduction, as we descend the animal scale. Moreover, in the embryonic stages of the higher animals—in those stages where integration is less complete—fission as an internal or as an external accident produces, not death, but double monsters or identical twins. So, also, if there be considered the suddenness of somatic death. The heart of a higher animal stops suddenly, and this accidental moment is named the moment of death. But the heart of a tortoise that has been in a dissecting dish for two or three days begins to beat as the knife reaches it. The watchers by the deathbed of a fish or a mollusk would continue to watch until putrefaction was obvious. I have fed a semi-putrid anemone with pieces of mackerel, and have seen a starfish with the tips of its arms rotten and pulpy crawl gayly about and evert its stomach on a pecten. In both of these cases the animals were moribund, and soon afterward actually died. It is a commonplace observation how rapidly the lower animals putrefy, and how necessary, to secure their histological preservation, it is to kill them as soon as possible after capture.

All this merely means that, in the first place, the stages of death creep over their simple framework so quietly that they are decaying before we know that they are dead: and, in the second, that protoplasmic death is more nearly identical with somatic.

Finally, in the descending scale, we reach unintegrated colonies of cells or single cells. Here there is identity between the protoplasmic individual and the soma; there is no somatic mechanism to break down, no chain along which protoplasmic death can creep from one cell to another.

It is clear, then, that somatic death is an attribute of the metazoa, a merely necessary result of their complexity of organization, and that somatic death becomes a more and more striking occurrence as the metazoon becomes higher and more complex. It is hardly necessary to point out that a line of argument precisely similar applies to somatic life, and that what are usually meant by life and death are merely somatic life and death, the merest mechanical results of complex mechanism.

It is true that the protozoa are untouched by somatic death, that it has and can have absolutely no place in their life cycle; that were somatic death the only death in the world, protozoa would be not potentially, but actually, immortal. It is equally true that no metazoan, however lucky, is at all likely to escape somatic death, is at all likely to be immortal. Even in the simplest metazoon, the life of the soma is bound up with the life of the units—or, at least, with the life of groups of the units—and the chances of protoplasmic death of any cell, or group of cells, being followed by somatic death, continually increases with the complexity of the organism, and increases in geometrical ratio as the number of component parts liable to accidental or intrinsic break-down.

The protozoa, then, and chains of single cell life, like the unicellular chain of germ cell life of which the metazoa are the carriers, may be called potentially immortal, and the metazoa mortal. But all this merely deals with somatic death, and is, so to speak, a biological accident. And so discussion or argument dealing with the possibility of immortal protozoa giving rise to mortal metazoa—by, for instance, some unequal fission separating a hypothetically mortal part of the plasma from a hypothetically immortal — is futile.

There is left, then, protoplasmic death. It is certain that protoplasmic death is not a necessary concomitant of protoplasmic life. We know of no means to secure the set of conditions which we call life, without the presence of some small piece of living material: we have considerable reason to suppose that spontaneous generation does not occur, at least as a normal factor in the organic world; we know with absolute certainty that it never occurs in metazoa. From this it follows that a direct continuous chain of cell life binds all animals with at least unicellular animals; that, for instance, we ourselves are at the end of a simple chain of cell division stretching through an inconceivable number of generations to an inconceivably remote past. So far as experience goes we can certainly say that living matter does not necessarily carry within itself the seeds of death. On the other hand, we can with equal certainty say that death apparently comes sooner or later to all chains of life; though it may be only that in chains of life, as in single lives, old age brings with it increased chances of death. Thus, if we review the procession of animal life in the past history of the earth, we find that always they are the topmost branches that wither; that sets of animals highest in the scale at any geological period are constantly dying out and being replaced, not by their own descendants, but by the descendants of a younger and fresher stock. I take it that this does not necessarily imply the otherwise improbable conception that death is a necessary incident in life. Just as the older a weapon or an ornament is, invariably it is the more battered or rusted, because, however jealously guarded, increase of years means increase in number of chances of exposure to accident; so, however nicely adjusted to environment, however constitutionally plastic and ready for adaptation to change, lapse of years must almost necessarily bring the one set of unsuitable conditions, the finally fatal moulding of the plastic constitution of an organic stock.

Exposure to accidents and somatic death are sufficient causes of death in the metazoa. It is not necessary to suppose that their cell protoplasm differs from the cell protoplasm of protozoa in having a limited duration of life.

Now let us turn to the two cases of practical immortality to see whether there be any special conditions.

First for the protozoa. It has been too easily assumed that in their case death follows from the gross accidents of life. The protozoa of the crag, or of the chalk, or of older beds, are not identical with living forms. In these beds, as in the ooze of existing seas, the invariable presence of large numbers of uninjured shells seems to point to protoplasmic death as having at least a possible place in protozoa. But existing protozoa as other existing animals are the direct descendants of the protozoa of long ago. They have established a weighty claim to be considered conditionally immortal. They enjoy two special advantages. By living singly they stand or fall by their own luck; they are not tied to the accidents of other cells in a cell commonwealth. Their easier contact with external media, and readier means of getting rid of waste products, or of accidentally taken dangerous substances, make them less subject to the intrinsic accidents associated with cell life.

Germ cells, again, unlike the working cells of the body, lie comparatively dormant within the tissues until the time comes for the playing of their own part. Practically, all the ova a woman produces are already formed in the new-born babe. They lie in the ovary, secluded from the wear and tear of cell life, subject to few dangers, and as far from the vicissitudes of somatic life as possible. And, lastly, the invariable extrusion of at least one polar body from every ovum is a mysterious and unique condition; a condition which may be a special effort to remove from the cell by-products the slow accumulation of which in tissue cells ultimately causes disease or disturbance.

We know that all other cells do ultimately die, and that death of all kinds can be reduced to cell death. The meaning of cell death and the meaning of cell life will be discovered together. But already we know something of the processes going on in living cells. In the first place there is decomposition. We cannot trace all the details, but we know that a number of substances are taken in, so to speak, at one end, and that a number emerge at the other. It is futile to say that the processes are not the processes of chemistry, as we do not know the constitutional formulæ of many of the substances which enter or emerge. But the sum of the activity is a slow feeding of substances of a high potential; a complicated flux of these within the cell; and an emergence of substances of a lower potential. Clearly, there are a large number of ways in which such a process can be disarranged. A sudden stimulus may set off the liberation of energy so rapidly that there is none left to do the work of bringing in more food, and starting the chemical reactions. Some waste substance produced accidentally or usually in small quantities, and not regularly excreted, may slowly accumulate until it clogs the whole action. Even in inorganic chemistry, reactions are often apparently so capricious that we know they must be influenced by a multitude of apparently trifling conditions. Lastly, the introduction of some unwonted substance may arrest or supplant the normal reactions.

For the elucidation of all these, there is wanted much experimental organic chemistry. Above all, there is wanted a careful comparison of the constitution with the effects of the different poisons which act on protoplasm. This is already leading toward a knowledge of the actual occurrences in a living cell, and it will, no doubt, get more and more clear that the events which cause protoplasmic death are simple interferences with the cell economy arising from internal or external accidents. As in bacteriology the idea that the fatal influence of these parasitic organisms is due to a struggle between life and life has been displaced by the idea that it is due to the direct action of a poison accidentally discharged by the parasites, so the causes of protoplasmic death will be attributed less and less to any inherent mortality in protoplasm. There seems no good evidence that protoplasm in suitable conditions, with sufficient food, should ever die, just as there is no reason that a flame, with a constant supply of fuel, and removed from violent draughts, should not burn from the beginning of time to the end of time. But the more complex flame of life is disturbed by many agencies, and these agencies cause protoplasmic death. In the metazoa, where each cell may suffer by the accidents of every cell, a new danger, the danger which is here called somatic death, is so urgent as to be fate rather than accident.—*Natural Science.*

BY the camera, with powerful telescopes, a new and large crater has been shown on the moon.

HOW TO PURIFY DRINKING WATER.

By Prof. PETER T. AUSTEN, Ph.D., F.C.S.

NEXT to air, water is the most immediate necessity of human life. Without air one can exist but a few moments; without water life cannot be prolonged more than a few days. The human body is largely composed of water, being about seven-eighths by weight of this substance. Water is being continually eliminated from the body in proportion to the amount that is taken in. This averages, including what is contained in our more solid food, at least two quarts a day. It is the agent by which the functions of the body are carried on, supplying many of its various wastes, and giving material for its processes. Indeed, a human being may be almost described as an animated pipe. Water is drunk primarily because it is water, and a certain amount of it must be daily taken into the system. It almost always contains extraneous matter, and this extraneous matter may or may not be harmful. The more nearly drinking water approaches to perfect purity, aside from a certain amount of dissolved gases which impart to it a slight pungency of taste, the better will it fulfill the office of a solvent in the body; the more easily will it be assimilated, the more easily will it pass or osmose through the membranes, and the greater will be the amount of solid substances that it will dissolve and eliminate.

Aside from the freedom of a water from dissolved mineral matters, and which make it "hard" or impart other properties to it, water may contain certain deleterious matters, which may cause it to become the means of imparting to those who drink it serious functional disorders, and often fatal diseases. It might be thought, in view of the care that is usually exercised in peeling, cleaning, and otherwise carefully preparing before eating vegetables dug out of the earth, as well as those that are not, that considerable care would be exercised in purifying the water that is drunk. But this is not generally the case. The ordinary surface well is a hole dug in the ground, and the water that oozes into it usually contains the dissolved impurities of the soil, putrefying vegetable and animal remains, as well as the pollutions from leaching cesspools and other similar abominations. So long as it is bright and sparkling it is considered both palatable and safe. But scientific investigation has shown that the sparkle of a water may be due to an excess of dissolved carbonic acid gas, and this condition may be the result of the putrefaction of organic matter; and that even when clear, sparkling, and palatable, water may still be superlatively dirty and deadly.

Biological investigation has established the fact that many diseases, such as typhoid fever, for instance, may be imparted by minute organisms popularly known as "germs," or more scientifically as "microbes." These minute organisms are given off by the patient suffering from the disease; and when they are transmitted to others, which happens in many ways, and very frequently by the agency of water as a conveying medium, they take up their abode in various organs of the human system. There, when the conditions are favorable, they develop, and live at the expense of their host, causing the functional disorders known as disease. Many sad instances of the effects caused by drinking polluted waters could easily be adduced. Water that is free from such pollution, but is simply turbid from suspended matter, as clay and the like, is unpalatable from its repulsive taste and appearance.

During the last few years the subject of water purification has received much attention, and successful methods have been introduced for filtering and purifying water on a large scale. Filtration on a small scale, while successful in many cases, comes, as a rule, under what is at present alleged to be housekeeping, and the success or failure of the method will, therefore, often depend entirely on the operative ability of some crude specimen of domestic home rule, a form of despotic government which has attained an extremely luxuriant growth in this country.

While I do not wish to undervalue any of the excellent small filters now on the market, I desire to explain a simple method by which any housekeeper of average intelligence can make an inexpensive contrivance which will do its work in a way not easily surpassed either in results, efficacy, rapidity or simplicity by any filter that can be bought. Such a filter can be set up in a short time at any place, and will be found particularly useful when one is away from home; for then special appliances are not always easy to obtain.

My attention was directed to this subject several years ago, and after some experimenting a simple apparatus was devised. Since then I have continued to experiment on this subject, and am more fully convinced than ever of the practical utility and efficiency of the method.

It has been known for many years that the addition of a minute amount of alum to a water containing bicarbonate of line in solution (and most natural waters contain more or less of this substance) will cause the formation of a gelatinous precipitate. This precipitate entangles and collects the suspended matters and germs, forming coagulated or agglomerated masses which are easily removed by simple filtration. Waters containing clay or mud which is so fine that a mechanical filter cannot remove it, when treated with a small amount of alum can be filtered perfectly clear through a coarse filter. The alum thus added is not left in the water, but is removed by the filtration, for its active constituent, the aluminic sulphate, is decomposed and precipitated by the action of the dissolved bicarbonate of lime. This should be well understood, although, if a minute amount of alum were left in the water its effects would not be noticeable, and even if present in larger amounts, it would not be at all dangerous.

The method of filtration is simple in the extreme. An oil bottle or any long, narrow-necked bottle serves for the filter. Tie around it a string soaked in kerosene, about half an inch from the bottom, set the string on fire, and hold the bottle bottom up. When the string is burned out, the bottom of the bottle is thrust into cold water. If properly done, this causes the bottom of the bottle to split off evenly. The rim of the glass should now be burned off a little with a round file to remove any sharp edges that may be left. The bottle is then thoroughly cleaned and placed neck downward in a convenient support, as, for instance, through a hole bored in a shelf, or it may be allowed to stand in a wide-mouthed bottle, resting by its shoul-

ders on the rim of the mouth. A small handful of cotton wool is now thoroughly wetted by squeezing it in water, and shreds of it are dropped into the bottle until a layer about two inches deep has been made. The shreds should be dropped in carefully, so as to distribute them evenly, and not to let them pile up in the middle or at the sides. When enough cotton has been dropped in, a cup or two of water is poured in and the bottle gently tapped. This consolidates the mass and finishes the making of the filter bed.

The amount of alum needed to coagulate the water sufficiently for filtering need not, as a rule, exceed two grains to the gallon, and in many instances may be less, but in certain cases of very dirty waters, such as that of the Mississippi River, the amount of alum may be increased to four or even six grains per gallon. The alum is best kept in a solution of such a strength that a teaspoonful of it will contain a grain. To save trouble, the following prescription will enable one to get enough of the solution put up at an apothecary's to last for a considerable time:

R. Alum	gr. 128
Aquæ dist.	℥ xvi

M. ft. solutio.

I may add that the expense of this prescription, including the bottle, should not exceed fifteen cents.

The treatment and filtration of the water is best done as follows:

A gallon of water is placed in a clean tin pail and two teaspoonfuls of the alum solution are added. It will save time to make, once for all, scratches on the inside of the pail, showing the height of one, two or more gallons of water. It is then well stirred and mixed with a clean tin dipper. It is best to keep this pail and dipper for this use alone. They should be kept scrupulously clean, and frequently well scoured with sapolio or a similar kind of soap. After mixing, the water is allowed to stand five or ten minutes, and then poured, by means of the dipper, into the filter. It will run through rapidly if the filter bed has been properly made, and will be as clear as crystal, and not seldom will form an astonishing contrast with the original water. The first half pint of the water passing through should be rejected. The filtered water may be caught in a pitcher or in any other convenient receptacle. A filter bed will last a day, but it is not advisable to use it longer. Each day the used filter bed should be thrown away and a fresh one prepared. The method may, of course, be applied to any of the many filters in use, by simply adding to the water to be filtered one or two grains of alum to the gallon. It will be a poor filter, indeed, that will not filter clear after this addition.

Of late, attention has been directed to the latent dangers in ice. It has been found that this apparently harmless and attractive substance may fairly reek with disease germs and filth of all kinds. Unless it is known from whence the ice comes, its use may be more dangerous than the use of water. Ice is sometimes derived from water which no one would think of drinking, as, for instance, from ponds in cemeteries and from rivers in the neighborhood of sewer outlets, and as a result may be indescribably foul. Aside from the danger of germs lurking in ice, there is risk in the indiscreet use of water cooled to an abnormally low temperature, since functional disorders are often caused by the drinking of very cold water. No water is so refreshing as that of a mountain spring, and one reason of this is that its temperature is just right. It is well to take hints that are given by nature, and the hint that the best temperature of drinking water is about fifty degrees Fahrenheit is a good one, and worth following.

I would suggest—and I am sure that every one who tries it will be more than satisfied—that the filtered water be caught in stoppered carafes, or, what is just as good, carefully cleaned sherry bottles stoppered with new, clean corks, and that these bottles filled with water and carefully stoppered be placed in the refrigerator for several hours. By putting half a dozen such bottles filled with water in the refrigerator and replacing them with others as they are taken out, a supply of clean, filtered water of a satisfactory and safe degree of coolness may be kept continually on hand.

The use of this simple method of purification of water will, I am certain, prevent many a case of sickness and not a few deaths, and it is so simple, cheap, and efficacious that any one can make a success of it.

COMPRESSED FOOD.

WHEN Uncle Sam is obliged to go to war again, compressed soups will compose an important part of the rations he distributes to the soldiers. In military operations the problem of transporting food is always of most vital consequence, the best army being of little use without a proper commissariat. Accordingly, there is urgent demand for some sort of provender that is at once highly concentrated and very nutritious. This want has so far been met that in future provisions for 10,000 men for ten days can be carried in five bullock wagons; or each soldier may carry sustenance for six days, the weight of which would only be one pound and a half.

At the opening of the Afghan war, fourteen years ago, an enterprising Englishman supplied the British army with a product of his own invention, in the shape of compressed pea soup. It was a yellow solid put up in four-ounce cans, one of which was a sufficient day's ration for a man. On the outside of the receptacle were pasted printed directions, saying: "Mix the contents to a thick paste with cold or warm water; then add sufficient water to make one quart of soup; boil until it is of the proper thickness, when it is ready for use." The British generals were so impressed with the merits of the preparation that great quantities of it were ordered for the army, and the inventor in England went into the business of producing it on a large scale, forwarding thousands of the four-ounce cans to India in pasteboard mailing tubes and otherwise.

When Gen. Roberts made his famous march to the relief of Kandahar, his troops were fed almost wholly on compressed pea soup. The rapidity of his progress was much facilitated by the small bulk of rations required to be taken along in this shape, a single mule being able to convey a day's provisions for a whole battalion. Subsequently, in the Zulu war, in the Egyptian conflict, and in the futile Nile campaign which had for its object the relief of Gordon at Khartoum, these canned rations were largely utilized, the profits on contracts for supplying them enriching the inventor. This and similar preparations will undoubtedly be the war foods of the future. There is no proprietary secret in the manufacture of compressed pea soup which should deter governments from producing it on a large scale, the process being very simple. The peas are steam-roasted and ground very fine, seasoning being added, together with a small quantity of beef extract to serve the purpose of stock. Finally the mixture is reduced to the smallest possible bulk by desiccation with dry heat and by pressure. A quart of good and thick soup made from peas will sustain any able-bodied man for twenty-four hours. Recently the business of preparing it for ordinary commercial purposes has been undertaken, and the compressed article can be bought by the 100 pounds at the rate of fifteen cents a pound. The quantity will make 400 quarts of soup, the cost of which is $15. It is put up for sale at retail in pasteboard boxes holding four ounces each, instead of cans. Tastes differ in various countries, and in England the formula for making it includes mint.

The manufacture of compressed soup of other kinds is about to be begun in this country on a large scale. Processes for preparing them will be similar to those above described, and they will be sold in solid form. Successful preliminary experiments have already been made with tomato soup, bean soup, mulligatawny, oxtail and beef soup. For all of these it will be necessary to use regular stock, together with which the various incidental vegetables will be boiled down to the lowest point of concentration, suitable seasoning being added. Finally the concentrated mixtures will be dried and packed in pasteboard boxes. Trial is to be made with the possibilities of compressed clam chowder, which should be a welcome novelty to the palates of shellfish-starved people who reside far from the sea.

With food supplies of this kind, campaigns of the future can be conducted with vastly less expense. Incidentally, reduced transport trains signify a reduced number of guards required for their protection. Compressed soups will admirably supplement the horse biscuits which European armies have already used with success, this latter invention enabling the trooper to carry in his haversack a week's rations for his charger, thus rendering him independent of forage. The value of peas as provender for soldiers has been recognized in the German army by the adoption of the erbswurst, or pea sausage; but this comestible does not suit the stomachs of British troops, and would probably not find favor in America.

The business of making and putting up soups in cans and bottles, ready for immediate consumption and only requiring to be heated before serving, has recently attained large importance in the United States. Many of them are sold in highly concentrated form, such as beef tea and broths for invalids, requiring to be diluted with seven times their own quantity of water. Thus a five-ounce bottle of chicken broth or consomme affords a full quart of soup. Ordinarily, however, these preparations are marked in bottles or cans holding quarts, pints, and half pints, not concentrated. They are as excellent in quality as French cooks can make them, and at three quarts for a dollar they are by no means dear, particularly from the point of view of the housewife whose husband complains that he does not get such soups at home as he can buy at the restaurants. There are two or three manufacturers who each put up 10,000 cans a day, two-thirds of them quarts.

A journey through one of the factories affords a most interesting and appetizing spectacle. Everything is as clean as a new pin. On the top floor all the raw material is delivered, in the shape of shins and other parts of beef suitable for the purpose, poultry, vegetables, etc. The meat, including the fowls, is put into a big cold storage room, whence it is taken as fast as it is wanted. In several huge copper pots, double jacketed for steam heat, stock by the hundreds of gallons is constantly boiling and bubbling. Huge chunks of beef, together with whole fowls, swim about in some of the pots, while in others are fowls only by the dozen. The stock for chicken soup, chicken gumbo, and mullagatawny must be made from chickens alone.

From these vessels the stock is drawn off through spigots, being strained incidentally into tin pails, by which it is transferred to other copper pots. In the latter it is boiled some more by steam, and various seasonings are mixed with it, according to the kind of soup that is to be made. From these mixing pots pipes extend downward through the floor to the room beneath, where young women stand at tables that are covered with rows upon rows of empty tin cans. Each girl has at her side several receptacles containing different vegetables. These have already been made ready for use. After cleansing, peeling, and cooking, they have been put into machines provided with dies, from which they are turned out by a sort of chopping process in all kinds of fancy shapes. For example, the carrots are cut into small cubes, or for julienne soup they are shredded, and there are ever so many other forms, which they, as well as turnips, onions, etc., are made to assume.

Now, if the vegetables prepared in this appetizing fashion were cooked with the soups, they would be reduced to shapeless and unpleasing pulp. Therefore, they are simply put into the cans at the last moment. For instance, the operator is engaged in filling cans with printaniere, which, as everybody knows, is one of the most delicious of liquid dishes. She takes each can and puts into it a certain quantity of carrots, peas, lettuce, and beans. So exact must be the proportions used that she weighs each contribution of vegetables on a pair of scales before turning it into the can. Being extremely skilled through practice, she is able to do this with surprising rapidity. Next she holds the can beneath one of the pipes that descend from the floor above, and turns a stopcock, permitting pure bouillon to flow until the can is filled. A little bag of muslin fastened over the end of the pipe strains the bouillon to perfect clearness. If she was putting up chicken gumbo or mullagatawny, she would draw pure chicken stock from another pipe, and similarly with other soups.

For each kind of soup different vegetables, always in measured quantities, are put into cans. Chicken and chicken gumbo must have a certain proportion of chicken meat in them, in the shape of little cubes, which are weighed out and added in the same fashion. After the fowls have been boiled in the copper pots to make stock, the meat is cut from them and chopped up by machine for this final purpose. For pea soup the peas are merely forced through a sieve and cooked with the stock at the beginning. But as was about to be remarked, the cans, as fast as they are filled, are removed to a long table, where their tops are soldered on by an expert young woman, who goes from one to another with a stick of solder in one hand and an iron instrument carrying a flame jet in the other. Not more than three seconds are required for topping each tin receptacle. Finally the closed cans are set in wire baskets, which are put for thirty minutes inside the queer-looking cylindrical ovens filled with steam. The steam kills all bacteria and spores of fungi that may happen to be inclosed in the cans, which might otherwise give rise to fermentation and putrefaction. Thus the soup should keep indefinitely, though now and then a can will swell at the top, showing that it has gone wrong.

All cans are manufactured on the premises, great care being taken to solder them only on the outside, because the muriatic acid used to make a flux is dangerous to health. For the same reason resin is employed instead of muriatic acid in soldering on the tops of the cans, after they are filled. Such pains are taken in this matter that there can be no possible danger from the cause suggested, but many people are so fearful on the subject that they will only buy soups in bottles. Accordingly, the same soups are put up in bottles holding exactly as much as the cans, but just double the price is charged for them when sold in glass, which, if it pleases the consumers, entirely satisfies the manufacturer. At one factory twenty different kinds of soup are put up in this way, varying in price at retail from $4 a dozen quarts to $8.50 a dozen quarts for green turtle and terrapin. The terrapin is made from real diamond backs, and not from "soliders" or mud turtles, with pure beef stock, sherry, and spices. After the usual style, the little black paws are left in, to show the epicure that the article is real.

There is a large class of people in every large city to whom ready made and ready cooked food is well nigh indispensable. In New York particularly—and like conditions prevail more or less in other great towns—thousands of families practice a kind of housekeeping so light that their cookery is practically limited to warming things up. Usually they follow this method of living because it is the cheapest possible, while in the frequent cases where both husband and wife work away from home it is hardly practicable to keep house on the conventional plan, and restaurants are expensive. As a rule they dwell in apartments of some sort, purchasing whatever provisions are most readily prepared. For them the delicatessen shop around the corner is a beneficent institution. There they can buy delicacies at all prices, from sausages up to a pate of quails with truffles. At the same establishment they can purchase every kind of freshly cooked meats, from a roasted turkey to a decorated boar's head. The available bill of fare includes all sorts of canned and preserved fish, meat, jellies, chicken and lobster salad, edible snails from France, cheese of every variety, imported vegetables in bottles and cans, mushrooms, dried vegetables, Saratoga chips, pigs' feet, and tripe. These are only a few of the delicious things available for a little ready money, and the list, supplemented with fresh eggs and bread and butter, will afford a varied and luxurious diet for any modest little family, particularly if the chief meal of the day be begun with a dish of ready prepared soup.

SUBCUTANEOUS INJECTIONS OF TESTICULAR FLUID.

IN a further communication to the Academie des Sciences, Brown-Sequard (*Les Nouveaux Remedes*, June 24, 1892) gives the results of three years' use of the subcutaneous injections of testicular fluid, especially in old men. He believes that the sexual glands have at least three distinct uses, consisting, 1st, in their *role* in procreation; 2d, in the influence that certain principles that are absorbed from them have on the nerve centers, giving the physical, moral, and intellectual characters proper to either sex; 3d, in a special tonic action, which increases certain active powers of the brain and cord. This last is the especial object of the study. As early as 1869 he advanced the idea that the sexual glands had internal secretions, which supplied useful, if not essential, principles to the blood. The first trial of the liquid in man was in the case of a well known savant, aged 72, who had become so weakened by age that he could barely work standing half an hour in the laboratory, and to whom mental work had become very difficult. Since 1860 he had been in the habit of measuring at short intervals the strength of the flexor muscles by the dynamometer, and from a maximum of fifty kilos, in 1860, it had fallen to thirty-seven kilos in 1869. He gained strength and mental vigor from the first injection, and reached forty-four kilos of flexor power. He could work for hours in the laboratory, and leave it in good condition to write memoirs on different subjects. The same patient, a short time ago, was moribund from the combined effect of an enteritis, a pertussis, and a rheumatic spasm of the muscles of respiration. A very strong preparation of testicular fluid was injected, with complete disappearance of all the alarming symptoms, which did not show themselves afterward. Other cases are those of a man, 89 years of age, who could hardly go up and down stairs in his house, but who was enabled to make long horseback rides without fatigue, after the injection, made by Variot, and whose improvement has continued for two years, the injections being made at intervals; of an old man dying from malarial cachexia at Port Louis, when Laurent injected the fluid from the testicles of monkeys, with the result that the patient arose from his bed the next day, after being confined to it for years; finally, of a young woman, wife of a Parisian physician, whom Gley rescued four times in five months from profound anæmia, the result of profuse pulmonary hemorrhage, by injections of testicular fluid. Special precautions taken in many cases show that the result was not one of auto-suggestion. Brown-

Sequard insists that the fluid does not act as a stimulant which calls into action pre-existing forces, to be followed by depression, but that it increases the transformation of energy to which are due the various powers of the spinal cord and brain.—*Therapeutic Gazette.*

THE OPPOSITION OF MARS.

THE *Times* of Sept. 3 contains a most important telegram, giving the results of Prof. Pickering's observations in Peru during the present opposition of Mars, which is one of the most favorable which has occurred during the last half of the present century. The work done at Arequipa in one respect contradicts and in others goes far beyond the results recently announced from the Lick Observatory. There can be no doubt that a considerable advance has been made by this year's results; many prior observations which have been considered doubtful have been confirmed and an additional interest lent to the observation of the planet.

The time, therefore, seems opportune for considering several questions connected with Mars, and it will be convenient to begin with the conditions of this year's observations, especially since the least astronomical among us has certainly noted with surprise the bright red star which now nightly rises low down in the southeast. Nor will he or she be less inclined to regard it when it is recognized as the planet about which during the last month so much has been written of human rather than of astronomical interest. If everything that one sees in print be true, the inhabitants of Mars are signaling to us, and it only remains for us to choose our manner of reply. Of course from signals the imagination of the ready writer has passed at once to words, and having got so far, each planet is about to become acquainted with the history and present conditionings of the other by means of a language understood of our neighbors as well as ourselves.

But first as to the cause of its excessive brilliancy during the last month or so, for this doubtless has had something to do with the present general interest taken in the planet. Mars was as bright in 1877, but

FIG. 1.—THE ORBITS OF THE EARTH AND MARS.

on that occasion nothing like the present amount of interest was taken in its movements and possible structure. For this there are two obvious causes—one the increasing interest taken by people in science generally; the other, popular glosses on several recent discoveries made regarding Mars itself.

The popular idea that the changes which have been recently observed on the planet are changes due to the work of its inhabitants—an idea based upon a mistranslation of a word—has, of course, generated the other one—namely, that vast operations have been undertaken for signaling purposes; and from this idea the step to Mr. Galton's or Mr. Haweis' method of signaling back is a small one. Small though it be, however, the public interest has thereby been greatly enhanced.

One of the most serious suggestions in modern times regarding signaling to bodies outside the earth we owe to a German astronomer, who some while ago enriched the world with the idea that the inhabitants of the moon might possibly be communicated with by establishing on the vast plains of Siberia geometrical figures, such as circles, etc., built up of fire signals, to which signal, if seen, the lunarians would reply by reproducing them.

Then the popular mind was content to bridge the chasm of 240,000 miles which separates us from the moon. But *now* Mars is the objective—Mars, which at its nearest approach is 35,000,000 of miles removed!

But Mars when in opposition may be very much further away than that; so far, indeed, that it is then observed to be one-fifth of its maximum brightness, and naturally with very reduced angular diameter. The two preceding oppositions at which its brightness has been at all comparable to its present one took place in 1860 and 1877, so that we find the most favorable oppositions about sixteen years apart. The reason of this will easily be gathered from Fig. 1, which shows with sufficient accuracy the very elliptic orbit of Mars in relation to that of the earth. The lines joining the two orbits are those connecting the two planets during some oppositions from 1830 onward to 1871. The outer planet, Mars, is represented nearly at the *perihelion* part of its orbit, that is the point at which it is nearest the sun (and therefore the earth, if we treat the earth's orbit as a circle), and the reason that the 1830 and 1862 observing conditions were so much better than those of 1869 and 1871 is at once clear. The opposition of 1877 and the present are not shown on the diagram, but they occurred at a time when Mars was not far from its perihelion.

The diagram also allows us to see that at the perihelion point of Mar's orbit the planet is very nearly at the time of the southern solstice, the north pole being inclined away from the sun. Also that this must occur about four months before the southern solstice of the earth, the direction of the axis of which is also shown.

So that at an opposition which occurs in August, as the present one does, we observe what happens in the summer solstice of the northern, and winter solstice of the southern, hemisphere of the planet. In fact, generally we have:

Time of opposition.	N. hemisphere.	S. hemisphere
August,	Winter,	Summer,
November,	Spring,	Autumn,
February,	Summer,	Winter,
May,	Autumn,	Spring.

The perihelion point of a planet's orbit is astronomically expressed by its heliocentric longitude, and the apparent size of its disk (on which its apparent brightness depends) by its semi-diameter in seconds of arc. Presuming that the longitude of the perihelion of Mars may be taken as about 334°, the following table will show how the great brilliancy of the planet in 1877 and the present year was caused; other less favorable oppositions are given for purposes of comparison:

Date of opposition.	Semi-diameter. ″	Heliocentric longitude of planet. °
1862, October 5	10·8	12
1869, February 13	8·2	145
1873, April 27	9·7	217
1877, September 5	**14·7**	**348**
1881, December 26	9·2	95
1884, January 31	8·3	132
1888, April 10	9·2	201
1892, August 18	**14·7**	**312**

So much, then, for the distance conditions. At its nearest approach the planet is 35,000,000 miles removed—let us say 150 times more distant than the moon.

We next come to the conditions of visibility. Mars is nearest to us (the degree of nearness depending upon its position in its orbit) when "in opposition," as we have said—that is, when it is in the south at midnight, and opposite the sun, the sun then being, of course, due north below the horizon. It will then appear to us "full," as the moon is said to be full when she occupies an analogous position. At this moment, then, the earth is invisible to the inhabitants of Mars unless she happens to transit the sun's disk.

The earth appears to Mars precisely as Venus does to us, and if inhabitants there be on Mars, and they study astronomy, a transit of earth to them will be what a transit of Venus is to us.

Further, as we see Venus as a half moon, and when nearer to us as a fine large crescent, so the Martians, as the earth approaches them, will see her as a half moon and then as a crescent, getting finer as the apparent diameter of the completed circle gets greater.

Mars, to see us best, must occupy a point near its perihelion. These things may be gathered from Fig. 2, in which an opposition at Mars' perihelion is shown, the orbits, but not the size of the bodies concerned, being to scale. Before the conjunction of the three bodies (in the line Mars, earth, sun) is approached, Mars will first have the earth as a half moon at a; this will gradually melt into a crescent till the moment of conjunction. Afterward the crescent will broaden, and its diameter will be reduced till the point a' is reached, when the earth will appear as a half moon again.

It is clear, therefore, that the earth will be a morning and evening star to Mars at the time of their nearest approach. The earth's crescent must not be too fine, or no observation will be possible on a dark background of sky. In other words, although we can observe Mars best when he is nearest, the privilege of seeing the earth when nearest to Mars is denied to his inhabitants.

We are now, then, in a position to discuss, so far as the mere conditions of visibility are concerned, the two suggestions as to earth signaling to which I have already referred.

Mr. Galton's proposal depends upon the observation that a "reflected beam of sunlight sent through a hole in a plate in front of the mirror was just distinctly visible as a faint glint at a distance of ten miles when the hole was a square of one-tenth of an inch in the side." He then adds: "The amount of fog and haze that a beam of light would traverse between us and Mars when the planet was high above our horizon could not exceed that along a terrestrial base of ten miles; consequently the same proportion between the size of mirror and the distance would still hold true. It follows that the flash from many mirrors simultaneously, whose aggregate width was fifteen yards, and whose aggregate length (to allow for slope) was say twenty-five yards, would be visible in Mars if seen through a telescope such as that at the Lick Observatory. With funds and good will, there seems no insuperable difficulty in flashing from a very much larger surface than the above, and sending signals that the inhabitants of Mars, if they have eyes, wits, and fairly good telescopes, would speculate on and wish to answer. One, two, three, might be slowly flashed over and over again from us to them, and possibly in some years, to allow time for speculation in Mars to bear practical fruit, one, two, three might come back in response. Dr. Whewell, if I recollect right, wrote a paper on the possibility of coming to an understanding with lunar inhabitants, if there were any. He would begin from the mathematical side. The practical difficulty is by no means insuperable of enabling many independent observers (who need not be near together) to direct their flashes aright. If mirrors could be mounted without much cost as heliostats (and perhaps they can be), it would be easy enough to do this. My own method is not practicable, at least without considerable addition and modifications, as it requires the object to be visible toward which the flash is directed, but Mars is not visible to the naked eye at day."*

Mr. Galton then uses sunlight and works in the day; Mr. Haweis, on the other hand, suggests electricity and night time:

"I infer from the astronomers that a signal on our earth about six miles in size of the nature of a bright light could be seen by the inhabitants of Mars, who by all accounts seem to be making the most systematic and herculean efforts to communicate with us by flashing triangular signals of presumably electric light. Why cannot we answer those signals by something which would resemble the lighthouse intermittent signal? Here is the method. London every night presents an area of at least twelve miles square brilliantly illuminated. That illuminating power might be enormously increased with only a few additional centers of powerful electric light. But without *any additional expense*, a little co-operation on the part of the gas companies would suffice to alternate darkness and light at intervals of five minutes over the whole of London between certain hours when traffic is more or less suspended. If only tried for an hour each night some results might be obtained. . . . We have actually the mechanism for interplanetary communication every night—why not use it?"†

Mr. Galton is careful to point out that his method of signaling requires sunlight, and that the signals are to be flashed to Mars in the earth's daytime; the moment of opposition, therefore, is at once out of the question. Even with the earth at either a or a' in our Fig. 2, the sun and Mars would be 90° apart, and in any case the signals would be visible to the Martians (if visible at all) on the part of the earth lit up by the sun. This does not seem a favorable condition, or at all events the most favorable one.

Mr. Haweis' plan secures a much stronger contrast. If it or something like it could be carried out, we can imagine the inhabitants of Mars studying the delicate earth crescent (with telescopes as powerful or more powerful than our own *bien entendu*), whether as a morning or evening star, and then seeing rhythmic flashes, reproducing the star included in the crescent of the Ottoman flag well within the horns of the crescent. Here we certainly get light on dark instead of light on light.

But there are other conditions of visibility besides those we have so far discussed. Supposing the whole

FIG. 2.—THE CONDITIONS OF VISIBILITY OF THE EARTH FROM MARS.

electric energy of London turned on Mars, would the volume of light be sufficient to produce a valid signal?

It is worth while, quite independently of the popular expectations of the present moment, to inquire into the actual conditions of the problem, telescopes on Mars as powerful as our own being always assumed.

If we are armed with a powerful telescope, under the best seeing conditions, first among which is its location at a considerable elevation, we may perhaps reckon upon using a power of 1,000, that is, the object is magnified a thousand diameters; in other words, it is brought a thousand times nearer. In the case of the moon, under these conditions any part of her we might choose to study could be examined, as if from London we were viewing it over Yorkshire with the naked eye.

The late Mr. Lassell, I believe, claimed as the highest achievement possible with his four foot telescope in the pure air of Malta, that if the lunarians were shaking a carpet as large as Lincoln's Inn Fields he could see whether it was round or square. This then would be the *ne plus ultra* in the case of a body 240,000 miles away.

Now, if we take the nearest distance to Mars as 35,000,000 miles, as I have stated,

			Miles.	
1,000,000 magnifying power would give us the power of studying Mars as if it were			35	away from us.
100,000	ditto	ditto	350	
10,000	ditto	ditto	3,500	
1,000	ditto	ditto	35,000	

We can put this differently. To the naked eye at the distance of Mars 1″ = 160 miles. Were Mars 1,000 times nearer, 1″ would become 0·16 mile. Now this at first seems very hopeful, for the exterior satellite of Mars has been seen in various telescopes.

We have already learned that the power employed last month at the Lick Observatory has not been so much as 1,000, but such that the planet has been brought within a distance of 50,000 miles. Under these conditions a line on Mars a quarter of a mile long will subtend an angle of 1″, or two lines a quarter of a mile apart should be separated and appear as doubles.

* *Times*, August 6, 1892.
† *Pall Mall Gazette*, August 13.

The second satellite to which reference has been made is only some 10 miles in diameter. We are justified by the visibility of the satellite, then, in saying that if a space 10 miles in diameter could be lighted up as brilliantly as by sunlight, on the dark hemisphere of the earth when Mars is above the horizon and at perihelion, it could be seen from Mars by telescopes equal to our own.

London, of course, is more than 10 miles in diameter, and we can imagine all the navies of the world with their search lights to flash simultaneously toward the planet, or to light up the clouds in a space as large as London, but there then will remain the question of the intensity of the light. What do electricians say is possible in this direction?

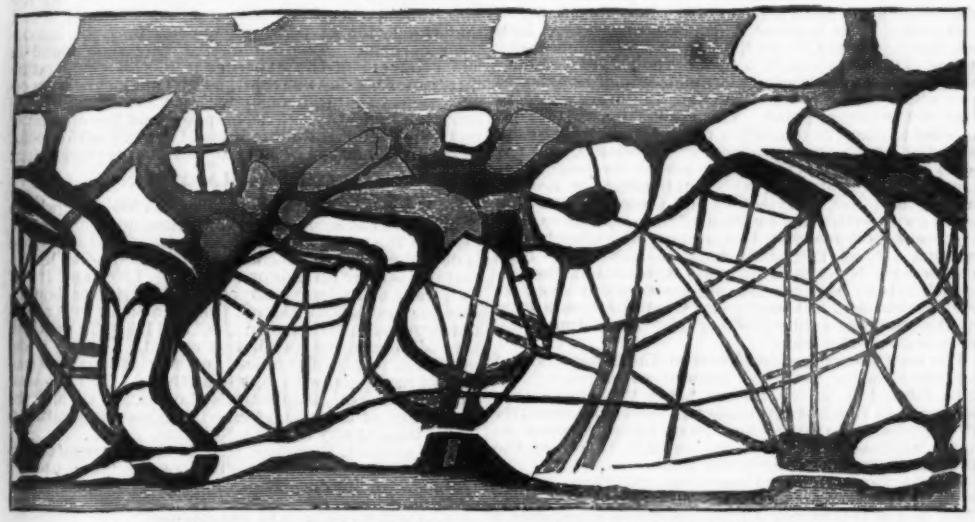
Whatever the answer to this question may be, it seems that signaling on Mr. Haweis' lines, light on dark, is a more hopeful proceeding than that suggested by Mr. Galton, and that on this system our conditions for reading signals are far better than those on Mars, as our dark hemisphere is much more exposed to our sister planet than is hers to us.

It is time now that we turn to those recent observations of our neighbor which have given rise to the ideas we have been discussing—ideas based upon the supposition that there is evidence which goes to show that the Martians are signaling to us by digging "canals" 1,000 miles long and 200 miles wide, and then doubling them, and in addition lighting numerous signal fires or flashing electric lights!

Here we approach a region of astronomical inquiry which requires no enhancement of its interest by the intrusion of popular delusions or imaginings, which, moreover, for the next few months, as details come to hand, will have all eyes directed to it.

It is not necessary to go further back than the year 1830 to appreciate the importance of the later inquiries. In 1830 Beer and Madler made an admirable series of drawings of the planet, which enable them to affirm the existence of fixed markings, and having fixed markings, not a long series of observations was necessary to determine the period of the planet's rotation on its axis.

In 1862 I (and many others) had no difficulty in recognizing the features on the planet which Beer and Madler had observed with smaller optical power



DOUBLING OF THE CHANNELS, OBSERVED BY SCHIAPARELLI IN 1882.

thirty years before. The instrument employed was a 6 inch Cooke achromatic, which I still hold to be one of the finest telescopes ever made. It enabled me to add details to those before noted, and the observations left no doubt on my mind that Mars had an atmosphere like our own; that its temperature did not vary many degrees from our own; that there were land surfaces and water surfaces; clouds and very obvious cloud drift; polar snows which melted with marvelous rapidity as the perihelion sun made its full strength felt. Further, that the changes in the appearances observed, especially in the lighter or darker shading, depended upon clouds and the smoothness or roughness of the water surfaces.

This latter conclusion I arrived at from the fact that the darkest markings, assuming them to be water surfaces, were more or less land-locked, and that changes in some of these surfaces were always most obvious close to the land. It was clear also that the rapid melting of the polar snow must be accompanied by tremendous inundations.

I append, as an example of the kind of work done on the planet with the small refractors generally available thirty years ago, some extracts from a memoir I communicated to the Astronomical Society at that time.* The large refractors employed added so far as I know very little.

"Although the complete fixity of the main features of the planet has been thus placed beyond all doubt, daily—nay, *hourly*—changes in the detail and in the tones of the different parts of the planet, both light and dark, occur. These changes are, I doubt not, caused by the transit of clouds over the different features. The effect of a cloudless and perfectly pure sky, both here and on Mars, appears to be that the dark portions of the planet become darkest and most distinctly visible; the coast lines (if I may so call them) being at such times so hard and sharp that (as has been mentioned by Mr. Lassell) it is quite impossible to represent the outlines faithfully; and this effect, be it observed, is completely distinct from the way in which the features grow upon one. MM. Beer and Madler remark: 'Generally some time elapsed before the undefined mass of spots seen upon first looking into the telescope resolved itself into recognizable parts.' This observation will commend itself to all who have observed such a delicate object.

"The effect of clouds, on the contrary, will be, I think, to make the *dark portions less dark* in proportion to the density of the clouds, and the *light portions lighter* in the same proportion. *It can never make a light portion dark.* If this be so, when we see a dark spot well defined, we can be sure that no clouds are above it, and that we actually see the planet itself; we cannot be sure, however (unless we are acquainted with the locality from previous observation), that dark spots do not underlie any of the lighter portions. Some instances of cloud transit were suspected by Father Secchi in 1858. Several unmistakable instances occurred during my observations. . . .

"But besides the cloud masses, which, as we have seen, obliterate the dark portions either partly or wholly, giving rise to different contours and tones, and rendering the actual features of the planet undistinguishable, the dense atmosphere of Mars, with its fogs and mists, appears to go for very much. I mention this more especially to point out that—although its effect was evident in the southern hemisphere in midsummer, upon the spots as they came on, and left the disk, as remarked by previous observers—it was much more evident in the northern hemisphere in midwinter, blotting out, as before remarked, *even on the central meridian*, all features north of + 30° latitude. This would appear to furnish another proof of extreme seasons on Mars, in addition to that supplied by the rapid melting and great extent of the polar snows, and to point out the desirability of taking advantage of all oppositions which happen as did those last year and in 1830, in the full summer time of the southern hemisphere, when the atmospheric conditions of the planet may be considered the best possible. With regard to this last point, it may be remarked that the southern hemisphere is the one which we shall ever be able to study best, in consequence of the great distance of the planet from us at those oppositions which occur when the northern one is turned to us.

"With regard to the green and red tints so often noticed on Mars, my observations have led me to hold the same opinions as to their nature as those arrived at by Father Secchi in his study of the planet in 1858. Nor do I think that it can any longer be doubted that—as he considered probable—the green and red portions do actually represent seas and continents, and are not the effect of contrast.

"The dark portions were noticed to be decidedly green in my instrument, both by myself and others, who observed Mars from time to time with me, the color being especially marked in Beer and Madler's spot *p n* (Drawings Nos. 7 and 8). In spite of the over-correction of my object glass, which should have 'reinforced' the red tinge, it was never sufficiently decided, I think, to suggest a contrast; and, indeed, the green was sometimes unmistakable when the red was not noticed, and when, therefore, there was no contrast to mislead the eye.

"Another point of agreement between the two series of drawings is not a little remarkable: the spots which were observed to be of a most decidedly dark tint in 1830 were darkest last year; and supposing the dark portions to be water, the darkest spots are those which are nearly, if not quite, land-locked. Passing on from the consideration of the general features of the planet, the snow zone next demands our attention. . . . Last year the solistice occurred on August 30, on the 23d of which month the snow zone was estimated to be one-fifth of the apparent diameter; by the 25th of the next month, September, this was reduced to about one-tenth, and again to one-thirteenth by October 11, when it was at times scarcely discernible; after which it began apparently to increase again.

"To the great eccentricity of the orbit of Mars, and the fact that the summer of the southern hemisphere occurs when the planet is near perihelion, is doubtless to be ascribed this very rapid melting of the southern snow zone, an observation confirmed by the much slighter variation in the dimensions of the opposite one. It appears to follow from my drawings, and I think also from those of Messrs. Beer and Madler, although they make no mention of the fact, that even at its minimum the center of the snow zone was not absolutely coincident with the planet's pole, being situated in somewhere about 20° of areocentric longitude (using Beer and Madler's start point), and in a latitude probably only a very few degrees from it. . . . The snow zone was at times so bright that, like the crescent of the young moon, it appeared to project beyond the planet's limb. This effect of irradiation was frequently visible; on one occasion the snow spot was observed to shine like a nebulous star when the planet itself was obscured by clouds, a phenomenon noticed by Messrs. Beer and Madler, recorded in their valuable work, *Fragments sur les Corps Celestes*. The brightness, however, seemed to vary very considerably, and at times, especially when the snow zone was near its minimum, it was by no means the prominent object it generally is upon the planet's disk."

We owe it to the illustrious Italian astronomer Schiaparelli that a world of wonders undreamt of thirty years ago now forms the chief subject of inquiry. His work was begun at the opposition of 1877, which, as we have seen, was as favorable as the present one, and continued during that of 1879–80. He showed that those parts of the planet which had been regarded by myself and others as the land surfaces, instead of being wanting in detail, as they had been seen, were really riddled by streaks, many of them very long and very straight, but in every case running toward a water surface, and in many cases connecting two water surfaces. These streaks he called *canali*, which in Italian, as *canalis* in Latin, means either a channel, a canal, or a pipe. Unfortunately, however, whenever it has been translated into English the word *canal* has been used, which, of course, with us suggests human labor. We have already seen what this has led to.

As a result of this minute inquiry rendered possible by his fine instrument (8¾ in. Merz) and perfect observing weather, a complete map of the planet with these channels was made.* But this was but the beginning of marvels. During the opposition of 1881–82 the work was continued, and now Schiaparelli, besides indorsing all the discoveries of 1880–81, found that in at least twenty cases the channels were doubled and consisted of two streaks 200 or 400 miles apart, instead of one. I append a copy of the sketch map he gave in his preliminary communication to the Academy of the Lincei.†

He distinctly stated that the doubling of these channels seemed to be connected with the time of the planet's year, and to occur simultaneously over the superficies of the planet which is supposed to represent land. When the opposition took place in August, that is in the full winter of the northern hemisphere, no trace of the doubling was visible, which is precisely what we should expect if the doubling depended in any way upon inundations caused by the melting of the northern snows, the north pole being turned away from the sun. The vernal equinox took place on December 18, 1881, and the opposition took place in the same month. The doubling of seventeen of these channels was observed between January 19 and February 19, that is, in the late spring of the northern hemisphere, which again is precisely what we should expect if they were connected with inundations.

Not only was this wonderful change noted, but here and there bright spots (previously noted by Green in 1877, and recalling Dawes' "snow island," seen in 1865) were recorded.

In the doubling of these water channels then, and in these snow-tipped hills, we have the origin of the "canal digging" and "fire signals" of which we have lately heard so much.

It will thus be seen that the widespread notions of the signals from Mars rest only on a mistranslation and upon the popular imagination running riot among the startling revelations of modern observers, among whom in this special line of work Schiaparelli must be acknowledged as *facile princeps*.

The observations which engendered invention in one class of minds engendered doubts in others, but the work of Perrotin and Thollon at Nice, in 1886, with the 15-inch refractor has completely indorsed the main points advanced by Schiaparelli with regard to the existence of the channels or straits. Two or three references to their published papers will show clearly what their view of the relation of them to the variously tinted parts of the planet really is:

"The triangular continent, somewhat larger than France (the Libya of Schiaparelli's map), which at that time stretched along both sides of the equator, and which was bounded south and west by a sea, north and east by channels, has disappeared. The place where it stood, as indicated by the reddish white tint of land, now shows the black, or rather deep blue, color of the seas of Mars. The Lake Moeris, situated on one of the channels, has also vanished, and a new channel, about 20° long and 1° or 1·5° broad, is now visible, running parallel with the equator to the north of the vanished continent. This channel forms a direct continuation of a previously existing double channel, which it now connects with the sea. Another change is the unexpected appearance about the north pole of another passage, which seems to connect two neighboring seas through the polar ice."‡

A short time afterward M. Perrotin stated that this same district of Libya had undergone a further change, the "sea" which had so recently covered it having retreated again for the most part, so that the appearance of the district was intermediate between that which it recently presented and that under which it was seen in 1886. Of the channels M. Perrotin has noticed four, three of which are double, which, starting from the "seas" of the southern hemisphere near the equator, and following a nearly meridional course, extend right up to the north polar ice cap, being traceable across the "seas" which immediately surround the latter.

Although Schiaparelli, as it will have been seen, connects the changes in the channels with the seasons of Mars, and although Perrotin and Thollon show their relation to the seas in their vicinity, other explanations of the phenomena have been suggested. Among these we must first refer to the view of Fizeau,§ that we were in presence of the results of glaciation on a tremendous scale, the parallel ridges being likened to crevasses or rectilinear fissures! It was imagined by him that relatively longer seasons and a lower temperature were capable of producing crevasses some thousands of miles long and hundreds broad.

But this was the only physiographic explanation offered. Mayeul Lamey, a Benedictine monk, ascribed the channels to volcanic action; to him they were the remains of enormous crater walls, and he states that

* Mem. R.A.S., 1865, p. 179.

* Osservationi astronomiche e fisiche sull' asse di rotationie e sulla topografia del pianeta Marte. R. Accad. dei Lincei, 1880-81.

† Memorie della Soc. Spettroscopisti Italiani, vol. ix., Dis. 6, p. 25.

‡ Abstract in *Nature*, May 21, 1886.

§ *Comptes Rendus*, June, 1883.

they are best seen when Mars reaches its most gibbous form and the angle of the incident light is greatest.

"Le plus souvent les astronomes se bornent à observer Mars vers l'époque de son opposition, c'est-à-dire de sa plus grande proximité de la terre; c'est, pensent-ils, le meilleur moyen de voir bien et de près les 'mers' de Mars. Si ces taches étaient des mers la raisonnement serait excellent, mais il n'en est pas ainsi. M. Schiaparelli a déjà fait la remarque que les canaux découverts par lui ont été observés non au voisinage de l'opposition mais un mois, deux mois après. Et pourtant la planète est alors bien eloignée déjà de nous. Pour moi, je constate également le même fait; je découvre tous les soirs un nombre de cirques de plus en plus considérable. La raison en est bien simple, du moment que les taches sont des ombres, ou du moins des parties réfléchissant moins la lumière. A l'époque de l'opposition, en effet, les rayons solaires tombent à peu près perpendiculairement sur la surface de la planète; Mars ne possède alors pas de phase, tandis qu'un mois avant ou après, la phase est très accentuée et les ombres deviennent possibles avec les élévations du sol."*

Another attempted explanation was that the channels were doubled in consequence of some play of diffraction. But enough has been said on this head; let us rather turn to the first fruits of last month's work.

At the Lick Observatory the channels were seen, and one of them was considered by three observers to be doubled.

From Peru we learn that Prof. Pickering saw many of the channels observed by Schiaparelli, but all were found to be single. The telegram adds, "not double, as stated by him;" but here is an error. We are near the southern solstice, as in 1877, and they were *not* seen double at that epoch. But even this is comparatively uninteresting after the revelations as to the effects of the melting snows.

Prof. Pickering discovered two mountain ranges in Mars to the north of the green patch near the planet's south pole. Between these mountain ranges the melted snow has collected before flowing northward. In the equatorial mountain regions snow fell, covering two of the summits, on August 5. On August 7 the snow had melted. "I have seen eleven lakes," the professor writes, "varying in size. These lakes branched out in dark lines connecting them with two large dark areas like seas, but not blue. There has been much local disturbance in the clouds round the planet since the snow melted, as is evident from the dense clouds which were concentrated within one area. These clouds were not white, but yellowish in color, and partly transparent. They now seem to be breaking up, but are still hanging densely on the south side of the mountain range. The northern green spot has been photographed."

Surely we have here the connection between the work of 1862 and 1877. The channels are true water channels; at one time at low channel we may have an unimportant stream like the low Nile; at another an ancient river bed, as it were, is filled to the utmost limit by the inundation. One requires to have seen an Indian river, or better still, the Nile valley, to realize what an inundation may mean, and especially under the conditions which have now been established to exist on Mars. But we may go further. A comparison of Schiaparelli's sketch of 1882 with his map of 1879 helps us considerably, and shows that we must take the effect of clouds over warm water into consideration. Two among the most undoubted and continuous water surfaces which I observed in 1862, which he has named Mare Cimmerium and Sabæus Sinus, were doubled also in 1882, and in my mind there is no doubt whatever that this doubling, at all events, is due to cloud banks lying or, rather, traveling longitudinally along the center of the water surface, precisely as the most magnificent cumuli which I have seen on this planet follow the equatorial current, entering the Caribbean Sea by Tobago. Obviously, by their lightness of shade, the channels are shallow, and they are only noticed in or near the tropics, so that the water must be highly heated before it empties itself into any of the southern seas.

Certainly it must be acknowledged that while the revelations show a remarkable similarity to our own atmosphere, so far as chemical structure and temperature are concerned, for the *onus probandi* lies with those who deny that they are dealing with the various forms of water, it would appear that the extremes of heat and cold are more generally operative in Mars than with us. The problem thus presented to us should prove interesting to the geologist. Was there any period in the earth's past history, or can there be in the future, which more resembles the present Martian conditions? Had we these enormous inundations, chiefly caused by polar snows melting? If not, were we sheltered from them by our more circular orbit and shorter year? Is Mars red because it is muddy? If so, what mud could give it the tinge we know?

J. NORMAN LOCKYER.

LUMINOSITY OF HYDROCARBON FLAMES.†

By VIVIAN B. LEWES, Royal Naval College, Greenwich.

EVER since Sir Humphry Davy, in 1817, advanced his celebrated theory as to the causes of luminosity of certain flames, the subject has commanded a great amount of attention, and the researches of Berzelius, Waldie, Volger, Hilgard, Landolt, Frankland, Stein, Heumann, Kersten, and more recently those of Soret, Birch, Stokes, and Smithells, have all added to our knowledge of the subject, but even now many points remain to be cleared up before we can gain a clear conception of the numerous factors and interactions which go to make up a luminous flame, and it is in hopes of raising a discussion upon some of these points that I venture to bring this paper before you.

Certainly, the two most important landmarks in the history of luminous flames are the original enunciation of the solid particle theory by Davy and Frankland's celebrated attack upon that theory in 1868. The relative values of the views of these two great experimentalists have been so recently discussed that it would be useless to again go through the mass of evidence which has accumulated during the past twenty-five years, and which seems to point conclusively to the luminous flame of burning hydrocarbons owing their power of emitting light to the presence of incandescent particles of practically pure carbon; while, on the other hand, I believe I am correct in saying that Dr. Frankland is unshaken in his belief that the cause of luminosity is to be found, not in solid particles of carbon, but in the incandescence of heavy hydrocarbon vapors.

In a paper read before the Chemical Society in January last, I described experiments and analyses which showed that the main reaction leading to luminosity in a flame containing hydrocarbons was the conversion in the inner non-luminous zone of the hydrocarbons into acetylene—a change brought about by the baking action of the heat evolved by the combustion in the outer layer of the flame, and in no way dependent upon oxidation, the change being entirely brought about by heat, as can be shown by passing the various gaseous hydrocarbons present in illuminating gases through heated tubes.

If an ordinary flat flame be taken and the gases be carefully withdrawn from the non-luminous zone and analyzed, the ratio of the acetylene formed, to the other unsaturated hydrocarbons, will be found to rapidly increase in their passage up the center of the flame:

Acetylene Formed in the Inner Zone of a Flat Flame from a No. 7 Bray Burner.

Portion of Flame.	Total Unsat. Hydrocarbons.	Acetylene.	Other Unsat. Hydrocarbons.
	Per cent.	Per cent.	Per cent.
⅛ in. from top of burner.....	3·565	0·115	3·450
1¼ in. from burner	2·063	1·303	0·760
1¾ " " "	1·393	1·133	0·260
2¼ " " "	trace	trace	——

Showing that by the time the top of the non-luminous portion of the flame is reached over 81 per cent. of the hydrocarbons present are converted into acetylene.

In such a flame luminosity commences just an inch and three-quarters above the burner, and if luminosity is caused by acetylene, it is natural to ask why should it not have commenced at an inch and a quarter—a spot where the acetylene is present in larger quantity than at the higher point. The reason for this is that in order to cause luminosity a temperature must be obtained at which the acetylene is dissociated, and the point at which this takes place marks the commencement of luminosity. If this temperature is not attained, then the acetylene in the presence of oxygen burns away with a non-luminous flame, or, if no oxygen is present, polymerizes into a number of higher hydrocarbons. If, on the other hand, a sufficiently high temperature is reached to cause dissociation, the liberated carbon becomes incandescent chiefly from its own combustion, but partly also from the combustion of the hydrogen liberated at the same time, and luminosity is the result.

The temperature needed to cause the dissociation of acetylene varies with the amount of dilution; the more diluted it is the higher being the temperature necessary to break it up, while the larger the quantity of acetylene the lower the temperature needed to cause its dissociation, and the smaller, therefore, the inner non-luminous zone of the flame.

In the flame from ordinary coal gas there is only from 1·1 to 1·3 per cent. of acetylene found in the inner zone, and in this highly diluted condition it requires a temperature of close upon 1,200° C. to break it up, while in other flames richer in hydrocarbons, and, therefore, richer in acetylene in the inner zone, the temperature required is not so high, for example:

	Flame of Coal Gas.	Flame of Paraffin Lamp.
Percentage of acetylene.....	1·133	2·222
Temperature at commencement of luminous zone.....	1,267°	1,060°

The fact that unless the requisite temperature is reached the acetylene burns away with non-luminous combustion is, I think, clearly shown by the following experiment:

The flame of burning alcohol contains more than half the quantity of acetylene that a good gas flame contains, and yet the combustion is practically non-luminous. If a dish containing some alcohol be placed under a bell jar, it burns with a flame having slightly luminous edges, and the temperature of the flame in this condition is 1,220° C. If now a glass plate be put over the mouth of the bell jar for a few moments, the mixing of the products of combustion with the air lowers the rate of combustion, and the temperature of the flame falls to 1,050° C., the flame becoming perfectly non-luminous; while, if oxygen be admitted to the bell jar, the combustion is increased and the temperature rapidly rises to 1,510° C., the flame becoming nearly as luminous as a coal gas flame, and depositing carbon on any cold surface held in it.

Again, if a small, luminous gas flame be allowed to play upon the bottom of a platinum dish, so much heat is abstracted from the flame that the temperature falls below the point necessary to break up the acetylene and the flame becomes non-luminous; while, if the flame is allowed to remain in contact with the metal until the dish is red hot, the luminosity returns, and the same effect may be produced by heating the interior of the dish with a blowpipe flame. (This experiment was, I believe, devised by Heumann to show the effect of cooling on luminosity.)

In every flame that I have examined which owes its luminosity to the presence of hydrocarbons, these bodies are converted into acetylene before luminosity commences in the flame, and every gaseous hydrocarbon which can, by heat, be made to deposit carbon, is converted into acetylene before any deposition takes place.

Many of the hydrocarbons present in coal gas are known to give out heat during their decomposition, but probably the most endothermic of these compounds is the acetylene, Thomsen having shown that no less than 55,010 units of heat are absorbed in the formation of 26 grains of acetylene, while only 10,880 disappear in the production of 28 grains of ethylene, and it is this which renders its decomposition by heat or, as shown by Berthelot, even by detonation into carbon and hydrogen so easy.

The fact that acetylene is formed from the gaseous hydrocarbons present in illuminating gas prior to the emission of light does not, however, by itself in any way disprove Dr. Frankland's theory that luminosity is due to very heavy hydrocarbon vapors and not to solid particles of carbon, as the well known tendency of the acetylene to polymerize into higher bodies might be taken as an explanation of the presence of sufficiently dense hydrocarbons in a flame given by say pure ethylene.

In order to determine whether or no the acetylene formed in the earlier stages of the flame renders the flame luminous by liberating carbon during dissociation as required by Davy's theory or by forming heavier hydrocarbons, a long series of experiments were made to ascertain the effect of heat upon the hydrocarbons present in coal gas, and by specially devised apparatus to measure the actual temperature to which the gases were being heated, and to collect and analyze the resulting products. These experiments are still proceeding, but I may state the main results of them as being as follows:

Ethylene passed at the rate of 4 to 5 c. c. per minute through a tube 2 inches in internal diameter undergoes no apparent change until the temperature rises to between 800° and 900° C.; it then breaks down to methane and acetylene; and, simultaneously with the formation of acetylene, heavy vapors appear which can be condensed, and in which benzine and naphthalene are conspicuous, as well as a heavy and highly fluorescent oil of high boiling point. Berthelot found that these substances were formed when acetylene was heated for some time at the temperature at which glass softens, and he also recognized among the products cinnamene, C_8H_8, and retene, $C_{18}H_{18}$, so that there seems very little doubt but that in passing the ethylene through a heated space in which the temperature does not rise above 900° C., methane and acetylene are first formed and the latter at once polymerizes into the heavier hydrocarbons. The amount of acetylene found in the products of the experiment of this temperature rarely exceeds one per cent., as the polymerization occurs almost simultaneously with its formation, but the proportion of ethylene decomposed and the amount of methane found in the products of decomposition point to the initial action of heat at this temperature being:

$$3C_2H_4 = 2(C_2H_2) + 2(CH_4).$$

On increasing the temperature from 900° C. slowly up to 1,000° C. the same action continues, no hydrogen appears in the products of decomposition, and no carbon is deposited, but the formation of oil and solid hydrocarbons is more abundant. Between 1,000° and 11,000° C. a trace of hydrogen appears and a little carbon begins to deposit with the oil, this increasing with rise in temperature up to between 1,300° and 1,400° C., by which time all formation of oil has ceased. Carbon is deposited in larger quantities, and the residual gas consists of hydrogen with a little methane. The temperature at which oil disappears is about 1,200° C., and the proportion of acetylene formed at this temperature is larger than at any of the lower ones.

It is evident, therefore, that if the temperature existing in the luminous portion of the flame exceeds 1,200° C., the luminosity cannot be due to such hydrocarbons as are formed from the acetylene, but that carbon will be present in abundance.

I have made several analyses of the carbon so deposited, and find that the hydrogen in it varies from one to three per cent., and, as there is no fixed ratio, I think there is not the slightest foundation for imagining that it is in combination; indeed, Stein's work upon this point is fairly conclusive, and any one who has attempted to prepare hydrogen-free carbon from sugar, or other similar body, will realize the intense power which freshly liberated carbon has of occluding and holding hydrogen.

Ethane, which is present in illuminating gas in larger proportion than is generally supposed, breaks up even below 800° C. into ethylene and hydrogen—

$$C_2H_6 = C_2H_4 + H_2,$$

and the ethylene behaves as before on increase of temperature. Methane, which is only present in very large quantities in the original gas, but is also produced during the decomposition of the ethylene, is apparently unaffected by heat until a temperature of 1,000° C. is reached, when hydrogen and unsaturated hydrocarbons begin to appear in small traces. Acetylene being the chief of these, this action continues and increases, so that at about 1,500° C. considerable quantities of acetylene are formed, which at once break down to hydrogen and carbon. With methane, as with ethylene, the first trace of acetylene found in the products is also accompanied by the formation of oils and heavy condensible vapors. Methane is not so easily converted into acetylene as the other hydrocarbons, and requires a higher temperature to bring about the change; but the fact that it does do so at high temperatures fully explains the facts first observed by Dr. Percy Frankland and Mr. Lewis Thompson, that although methane when burnt by itself has but a very feeble power of illumination, yet when used as a diluent of heavier hydrocarbons it greatly increases the light-emitting power of the flame.

In order to apply the knowledge these experiments give of the changes taking place in the hydrocarbons present in coal gas during their flow through the heated channel in the interior of the flame, it is necessary to map out the temperatures existing in a luminous flame, and the flat flame from a Bray burner (No. 7) lends itself conveniently to this purpose.

Using the Le Chatelier thermo-couple described by Dr. Roberts-Austen at the last meeting of the British Association, and making only one short twist of the wires to give contact, and then coating them with hard glass, it is possible for nearly a minute to get the temperature in the portion of the flame in which the short twist is plunged, and if thin wires are used the loss by

* "Note sur la Découverte du Système Géologique Eruptif de la planète Mars." Par Fr. Mayeul Lamey, O.S.B. Autun. (Dejussieu, 1884.)

† Read before the British Association, Edinburgh meeting, 1892, chemical section.

conduction is very small. Of course, if part of the flame through which the glass-coated wires pass is at a higher temperature than the portion around the twist, as the heat penetrates the glass it will give too high a reading, but for the first thirty or forty seconds there is no fear of this.

With this arrangement I was able to map out the temperatures existing in the flat flame, in which I had traced out the changes taking place in the constituents of the gas; and I found that the temperature rapidly rose from 500° C. half an inch above the burner to a little over 1,200° C. at the commencement of the luminous zone—the luminous edges having a temperature of 1,216° C.; and these temperatures were further increased in the luminous zone, until near the top of the flame 1,368° C. was reached. This at once gives us the secret of the luminosity. From the mouth of the jet and up to a temperature of 1,000° to 1,200° C., we have the formation of acetylene from the original hydrocarbons; but the moment the requisite temperature is reached by the combustion of the hydrogen and the carbon monoxide, the acetylene decomposes with a further rise of temperature, and the carbon, heated to incandescence, radiates heat and light.

In the experiments with the hydrocarbons the gases were passing through a heated space six inches long kept at a constant temperature; in the flame they are passing through a space 1·75 inches long with a rapidly increasing temperature, so that instead of the acetylene becoming polymerized to any great extent, it reaches a temperature at which dissociation takes place almost immediately it is formed.

Experiments with hydrogen-borne benzene vapor show that at a temperature of 1,200° to 1,300° C. it is reconverted to acetylene, and this then at once breaks down to carbon and hydrogen, and the same happens with any other heavy vapors formed in the inner zone, and these bodies formed probably in minute quantities by polymerization, together with any remaining methane, are reconverted into acetylene and deposit their carbon as soon as the necessary temperature is reached, and this supplies a fresh quantity of carbon particles to the upper portion of the luminous zone, and so increases its size.

The careful analyses of Hilgard, Landolt, and Blochmann, made to determine the interactions taking place in various flames, are in their main features in close accord with a long series of analyses of the flame gases which I have made, and one of the most striking features of these is the comparatively slow rate at which the heavy hydrocarbons disappear, as compared with the other constituents of the original gas. This I find to be chiefly due to diffusion, which causes the hydrogen and methane to find their way to the outer skin of the flame first, and it is these bodies, therefore, which bear the brunt of the earlier combustion and yield the heat which bakes the heavy hydrocarbons ascending straight upward from the burner into acetylene.

If three concentric tubes be placed half an inch above an open tube from which coal gas is issuing, and the gas from each is withdrawn and analyzed, it will be found that on comparing them with the original gas the action is most marked. The gas in the outer tube being practically a mixture of air with hydrogen and methane and no heavy hydrocarbons, while in the inner tube the unsaturated hydrocarbons have risen from 3·56 to 4·53.

All analyses of the flame gases also show that the hydrogen burns first, and that next in rapidity of burning is the methane, and Prof. Smithells advances the theory that in the incomplete combustion taking place in the flame the hydrocarbons burn to carbon monoxide and hydrogen.

As has been already shown, the proportion of heavy hydrocarbons consumed before conversion into acetylene and then dissociation into carbon and hydrogen is very small, and the principal hydrocarbon undergoing combustion in the flame is methane. The analyses of Landolt, Hilgard, Blochmann, myself, and Smithells all show that a large increase in the carbon monoxide undoubtedly does take place; the higher one ascends in the flame until the luminous portion is reached, and also it is probable from the same analyses that hydrogen is generated by some action in the outer layer of combustion, as in spite of its rapid combustion at the base of the flame, as shown by the large percentage of water vapor formed there, it keeps reappearing for some little distance up the flame, and it is to explain the joint appearance of carbon monoxide and hydrogen that Prof. Smithells brings forward the theory that carbon burns to carbon monoxide, setting the hydrogen free from the hydrocarbons.

There is not the slightest doubt from the work done by Dalton, Blochmann, Kersten, E. von Meyer, and Dixon, that if you explode together gaseous hydrocarbons with a limited supply of oxygen, you obtain carbon monoxide and hydrogen, and it is also probable from H. H. Baker's experiments that carbon burns first to carbon monoxide, but I fail to see that any such theory is necessary to explain the actions taking place in a flame. If we analyze the gases present in the flame ¾ of an inch above the burner, which is about the point at which increase of carbon monoxide commences, although it is not marked until 1½ inches up the flame, we find that in the outer zone of the flame we have—

Water vapor	24·65 per cent.
Methane	8·35 "

A little deeper in the flame we get—

Water vapor	17·94 per cent.
Methane	21·51 "

and from this point up to 1½ inches you have the temperature rapidly rising from 750° C. to 1,100° C., and it is a well-known fact that if heavy hydrocarbons and water vapor are heated together they yield carbon monoxide and hydrogen; indeed it is the most expensive method of making water gas, and wrecked one process for the manufacture of illuminating water gas, and one would expect the same thing to happen when methane and water vapor are heated to this temperature.

In order to try if this were so, methane was passed through water just below the boiling point and then through a tube heated to 1,000° C., the resulting gases being then analyzed, when it was found that the methane and water vapor had interacted to form hydrogen, carbon monoxide, and carbon dioxide, and I should think that this is a far more likely cause of the effects produced in the flame.

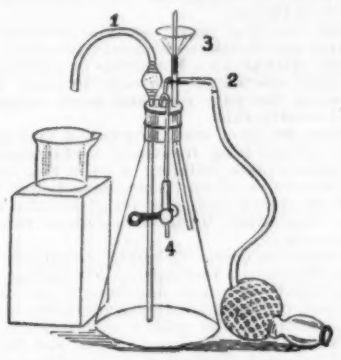
Taking the facts which I have now laid before you, we may, I think, fairly fully explain the actions that take place in the flame and lead to luminosity. As the gas leaves the jet, the hydrogen rapidly diffuses to the outer edge of the flame and burns, the methane doing the same but rather more slowly. The combustion of these gases raises the temperature 500° C. in the first half inch, while before another half inch has been traversed 1,000° C. is reached, and the chemical changes in the hydrocarbons are progressing rapidly, the unsaturated hydrocarbons and higher members of the saturated hydrocarbons being rapidly converted into acetylene. If the temperatures of the flame were not allowed to rise above 1,000° C., this acetylene would be nearly all polymerized into benzene, naphthalene, diphenyl, and other complex bodies, which would be slowly burnt up without liberation of carbon, and a non-luminous flame would result. In the case of a gas flame, however, instead of remaining at 1,000° C. the temperature rapidly rises to 1,200° C., with the result that, instead of polymerizing into more complex bodies, most of the acetylene formed at once splits up into carbon and hydrogen, and the former heated to incandescence by combustion gives the luminosity. It is the attainment of this temperature that marks the limit of the non-luminous zone. If this were the only action, however, the luminous zone would be very short. The bodies, however, formed from the acetylene before 1,200° C. was reached, and the methane, of which some still remains unburnt, are converted into acetylene at a still higher temperature, *i. e.*, 1,300° C., and this, being reached near the top of the luminous zone, yields a fresh supply of carbon, and so increases the height of the light-yielding portion of the flame.

In producing the luminous flame, therefore, there are two main factors to be observed: first, to use such compounds as shall be most easily converted into acetylene, and secondly, to attain as quickly as possible as high a temperature as practicable.

A FILTER FOR "MAYER" ESTIMATIONS.

By F. C. J. BIRD.

WE have a conical flask fitted with a rubber cork, through which passes tube 1, shaped as shown, the bulb being filled with asbestos; 2 is the bellows of a spray producer, and is connected with a tube which passes into the air space of the flask. In 3 we have a



funnel closed with a stopper, the limb of the funnel being bent to an angle and diagonally as shown. The fourth tube (4) is closed with a burette clip. In working, the "Mayer's" solution is run in through the funnel into the alkaloidal solution until precipitation apparently ceases; then some of the mixture is squeezed out with the bellows through tube 1, thus enabling the operator to judge, by testing the clear liquid driven into the beaker, whether the end point has been reached or not.—*Chem. and Drug.*

THE PRODUCTION OF HEATING GAS AND AMMONIA.

By A. HENNIN, M.A.I.M.E.

IN the dry distillation of coal, the production of ammonia does not exceed 10 per cent. of the total nitrogen, being limited by the water of combination of the coal. This conclusion is corroborated by the fact that in blast furnaces where raw coal is used, although all the coal is consumed, and consequently all the nitrogen is liberated, the product of ammonia does not exceed 15 per cent. of the total nitrogen. In addition to the water of combination in the coal, there is doubtless also a proportionate effect, due to the water of combination of the iron ore and flux, perhaps slightly influenced by traces of water carried into the furnace with the air blast.

If all the nitrogen in coal could be converted into ammonia, a coal containing 1¾ per cent. of nitrogen would produce per ton the equivalent of 164 pounds of sulphate of ammonia. This result is not obtainable in blast furnaces; it is impossible in the manufacture of illuminating coal gas or of coke; but it is realizable to a large extent in the manufacture of heating gas for metallurgical and other uses.

The nascent hydrogen from the water of combustion of the coal is the main element which accomplishes the transformation of the nitrogen into ammonia. Hence, the more water, the more ammonia. If a sufficient outside supply of water is given, all the other conditions being favorable, the total amount of the nitrogen may be secured as ammonia. Practice has demonstrated the correctness of this view. In 1877, Dr. H. Grouven, of Leipzig, discovered that in a large excess of superheated steam, when the necessary conditions of temperature, time, and contact are supplied, combined nitrogen is transformed into ammonia. He applied his discovery to an analytical method of nitrogen determination, which gives concordant results with other accurate methods. Later on, Beilby, Foster, Guegen and others demonstrated that the coke, which retains a large percentage of nitrogen even at a light red heat, parts with it in the form of ammonia in the presence of a large excess of steam at an intense heat. Since, in accomplishing this result, the steam is decomposed by carbon, liberating hydrogen, and the carbon is transformed into carbon dioxide and carbon monoxide, it is evident that we have here the elements of a process for producing simultaneously large quantities of ammonia and also heating gas of high calorific value. Between this theoretical reaction, however, and the establishment of an economical manufacturing process, there were many difficulties to overcome. As has been pointed out, the main conditions for the formation of ammonia are eminently favorable to the production of a good heating gas. But high temperature is not compatible with a large excess of steam unless the latter be intensely superheated; and such a temperature cannot be maintained for any considerable time without an outside supply of oxygen (or an expensive external heating. Moreover, steam in large excess is liable to produce a gas very high in carbon dioxide, and hence of comparatively little calorific value. Nor should it be forgotten that ammonia, under favorable conditions, is decomposed at a temperature far below that which is required to decompose water and reduce the carbon dioxide into carbon monoxide. The practical operation, therefore, must be so conducted as to maintain the proper heat, make the conditions for the decomposition of ammonia as unfavorable as possible, and, at the same time, produce such a gas as is required in the metallurgy of iron and steel where rapidity of heating and melting, with the lowest percentage of waste, is a prime consideration.

I have found that when high pressure steam is moderately superheated and evenly distributed and diffused in the glowing mass, a limited supply of air, drawn into the generator, is sufficient to maintain the temperature needed to admit continuously from one to one and three-quarters pounds of high pressure steam to the pound of coal, and that this proportion of steam is ample to provoke the necessary reactions which transform into ammonia 50 to 60 per cent. of the total nitrogen of the coal, and still to produce a gas of the following composition:

	Per cent.
Carbon dioxide	10·50
Oxygen	1·00
Carbon monoxide	20·00
Methane and homologous compounds	4·50
Hydrogen	38·00
Nitrogen	26·00
	100·00

This gas is rather high in carbon dioxide, but the total of combustible matter is considerable, and the efficiency of the gas in a regenerative furnace for heating iron is very great. It burns with a sharp, white-bluish flame, not without luminosity, and heats more rapidly and more economically than the ordinary Siemens gas.

It would seem, at first glance, that the admission of 1 to 1·75 pounds of steam to the pound of coal would have a high cooling effect, hindering the steady production of a gas of the above composition, if the carbon is taken as the only source of heat. Practice shows, however, that such is not the case. There are different ways of accounting for this result. In the first place, independently of the heat furnished by the high pressure superheated steam, many reactions producing heat occur, such as the formation of the ammonia, etc. On the other hand, the gas being exhausted at high speed, the operation takes place under a constant partial vacuum. It is presumable that, under such conditions, the chemical reactions to which we are accustomed are modified, and that we are in the presence of phenomena of physical and thermochemical order, which actuate the distillation of the volatile matter, promote it at a low temperature, and generally facilitate the reactions. Whatever the explanation, the actual result is, that not only there is no cooling effect in the continuous production of the above gas, but that the temperature has a tendency to increase in the zone of combustion. If the steam has been sufficiently superheated and diffused thoroughly in that zone, it is practically all decomposed during the operation; and the reactions proceed without intermission, the liberated nitrogen being transformed in the presence of nascent hydrogen into ammonia, while the resulting fuel gas, as the above analysis shows, is still high in hydrogen.

The danger of dissociation of the ammonia is not as great in practice as has been anticipated. If we remember in what infinitesimal proportion the ammonia is present, diluted, and hence practically protected by the other gases, it is evident that the chance for decomposition, with ordinary care, is very small. And if the product is instantaneously exhausted from the hot atmosphere, carried into the cooler zone immediately above, and led out through green fuel on the top, the ammonia is secured.

With well devised producers and the proper plant, when the art of managing and controlling the heat in the different zones of the fuel bed has been mastered, there is no difficulty in producing regularly, with the proper amount of steam, from each ton of coal, 70 to 80 pounds of sulphate of ammonia; 130,000 to 150,000 cu. ft. of heating gas of high quality; and, in addition, some 15 to 20 gallons of tar, according to the nature of the coal.

The generator must be perfectly accessible, so that it may be easily cleaned, and the coal may be perfectly broken through. The aim should be to keep the fuel like a sponge, but without holes or cavities. The advantages of a good stirring are obvious; it insures the complete diffusion of the steam through the glowing mass, maintains the high temperature in the lower part of the generator, and keeps the top cool, thus tending to secure economy in coal, the minimum of necessary cooling surface in the condensers, and the maximum yield of ammonia. The carbon dioxide should be kept at a minimum. Its formation is a function of temperature; the lower the temperature at which the carbon and air or the carbon and steam come in contact, the more carbon dioxide is formed. It must not be forgotten that the depth of the fuel has a very important bearing on the condition and results.

In order to obtain good heating gas and the largest yield of ammonia and tar, the gas must be well cooled and well scrubbed. If the gas be not cooled, the

quantity of water which it will suck from the washing apparatus is so great as to destroy the value of the gas for heating purposes, while the ammonia, even if it be in the form of fixed salt or sulphate, is carried away, and the more volatile constituents of the tar are carried forward and settle in the conduits.

The system is entirely smokeless, and has the further advantage of taking the sulphurous acid very thoroughly out of the gas.

TESTS FOR VEGETABLE WAXES.*

THE chief varieties of vegetable wax, among which are included certain substances known as "vegetable tallows or fats," comprise the following:

1. *Carnauba wax*, called also Ceara or Brazil wax, from *Copernica cerifera*.
2. *Pela wax*, or Chinese wax, from *Fraxinus chinensis*.
3. *Sumach wax*, or Japan wax, from several species of *Rhus*.
4. *Kaga wax*, from *Cinnamomum pedunculatum*.
5. *Ibota wax*, from *Ligustrum ibolu*.
6. *Stillingia tallow*, or Chinese vegetable tallow, from *Stillingia sebifera*.
7. *Myrica wax*, or myrtle wax, from *Myrica cerifera*.
8. *Orizaba wax*.
9. *Wax from stick lac*.
10. *Bahia wax*.

Alcohol, ether, chloroform, petroleum spirit, and alcoholic solution of potash exert a solvent action on the various kinds of wax met with in commerce, and the behavior of the solutions with solution of ammonia and with alcoholic solutions of lead acetate and of ferric chloride has formed a means of distinguishing them from one another. The process is as follows:

A sample of the wax is heated with ten times as much chloroform to boiling, and, when completely dissolved, cooled in cold water.

1. The chloroform solution remains clear after cooling.

A. Ether dissolves completely.

(*a*) Alcoholic solution of ferric chloride gives, with the alcoholic solution of the wax, a precipitate insoluble on heating—*wax from Myrica quercifolia*.

(*b*) Ferric chloride colors alcoholic solution black—*wax from undetermined species of Myrica*.

(*c*) Ferric chloride colors brownish but gives no precipitate—*wax from Myrica cerifera; wax from Orizaba*.

B. Ether dissolves only a part. A sample is boiled with ten times the quantity of alcoholic potash solution till saponified, and the soap heated with 100 volumes of water.

(*a*) The soap is completely soluble—*Japanese wax*.

(*b*) The soap is partially soluble—*African beeswax*.

2. The chloroform solution becomes cloudy on cooling.

A. Alcoholic solution of acetate of lead gives, with the alcoholic solution of the wax, after a few minutes' standing, a cloudiness—*wax from stick lac*.

B. Alcoholic solution of acetate of lead gives no cloudiness.

(*a*) The ethereal solution of the wax becomes cloudy on the addition of an equal volume of alcohol—*Brazilian wax*.

(*b*) The ethereal solution remains clear—*Bahia wax*.

ESSENCE OF LEMON.

By ARTHUR A. BARRETT, Messina.

IN making oil of lemon, one man takes a lemon in his hand, and with three rapid strokes with a large knife cuts off nearly all the peel in three slices. The central portion which is left consists of most of the pulp with a little of the peel, top and bottom. This is simply pressed for making lemon juice. The slices pass to a second workman, who sits on a low chair with an ordinary common quality bath sponge, worth about 6*d*., in one hand. With the other he presses the slice of peel against the sponge, pressing the edges of the peel only with his fingers, the object being to press the convex piece of lemon peel as nearly flat as possible. One man can extract about 1½ lb. essence of lemon per day. To insure the cells being fully charged with moisture, it is usual to allow the lemons to stand in water for a short time. After referring to the preparation of lemon peel for candying, the author said that the peel is sometimes deprived of its oil without rupturing the oil vessels, and the quantity of essence obtained in this way is considerable. It is generally assumed in England that all pure essence of lemon is good. This is far from being the case. He had seen essence of lemon containing 15 per cent. of turpentine which was really superior to essence of lemon made the same day in his presence, and absolutely pure. This results from the extraordinary variation in the quality of the essence made in the various months. This difference is not noticed much in England, even the best exporters having to make an average sample, which they can supply all the year round. Turpentine is in large use, and is purified in a peculiar way. One exporter is said to use 10 tons per annum. Strange to say, the worst qualities of essence all go to London, Manchester, and Glasgow. In particular English wholesale druggists have an unenviable reputation for buying low qualities. Experienced buyers claim to be able to distinguish the district and village in which an essence had been made simply by smell and inspection. Testing is carried out as follows: A sample is poured out into a tumbler and shaken up after placing the hand on top. Great attention is then paid to the duration and size of the bubbles and froth, the color is noted, and one smell is taken with the glass full and another after emptying it. Turpentine will certainly be detected in this way if over five per cent. is present.

NEW FORM OF CARBON.

A NEW variety of carbon has been obtained by Luzi. When a piece of porcelain, such as a crucible or crucible cover, is heated in a blast furnace to the highest attainable temperature, say to 1,770° C., the fusing point of platinum, and then the access of air is cut off, the highly-heated porcelain is surrounded by a smoky flame, which is allowed to act upon it for ten or fifteen minutes. On removing the piece of porcelain from the furnace, it is found to be covered with a peculiar deposit of carbon. If it were unglazed the deposited carbon resembles graphite, but if glazed the deposit is bright and silvery and has a metallic luster, resembling closely a silvered mirror. Portions of the deposit adhere so firmly that they can be polished with a cloth; others separate in the form of mirror-like facets of high luster.

Loose portions can be pulled off, and then they curl up into rolls like metal shavings. They are exceedingly light and stick to the fingers like silver leaf. This form of carbon is free from ash and does not contain hydrogen, nor does it give the nitric acid reaction for graphite. Moreover, it is absolutely opaque. The author has also examined seventeen additional varieties of natural graphite by his nitric acid reaction. Nine of these give the action and are therefore graphite proper; eight do not, and are therefore included in the second class, which he calls graphitite. Of the thirty-one specimens thus far examined sixteen were graphite and fifteen graphitite. The blackish-gray substance into which diamond is converted on heating, and which resembles graphite, did not give the nitric acid reaction.—*Ber. Berl. Chem. Ges.* XXV., 214, February, 1892; *G. F. B., in Amer. Jour.*

* From a paper in the *American Druggist*.

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